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# MIND

A QUARTERLY REVIEW  
OF  
PSYCHOLOGY AND PHILOSOPHY

EDITED BY

PROF. GILBERT RYLE

WITH THE CO-OPERATION OF PROF. SIR F. C. BARTLETT AND PROF. C. D. BROAD

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# MIND

## A QUARTERLY REVIEW

OF

## PSYCHOLOGY AND PHILOSOPHY

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### I.—SOME REFLECTIONS ON REFLEXIVITY

BY JØRGEN JØRGENSEN

IN this paper I wish to advance the view that so-called reflexive phenomena does not exist. Especially, I wish to defend the following theses :

- (i) There are no reflexive relations or any other so-called reflexive phenomena.
- (ii) In particular, no language or linguistic expression is self-referring.
- (iii) The paradoxes of the Epimenides-type can be solved and explained in a quite elementary way without any need for a more or less complicated theory of logical types.
- (iv) A simple form of such a theory seems necessary, however, in order to explain the syntactical use of the word "all". By means of this theory various paradoxes, *e.g.* Russell's paradox, are eliminated and explained.

My conscious motives for trying to defend these theses are as follows :

First, I have always felt a certain disproportion existing between the complicated means used to avoid the paradoxes and the relative simplicity of the paradoxes themselves. It is generally thought foolish to dynamite butterflies, and as compared with the simpler paradoxes at least, the various theories of logical types seem to me to be of a too subtle character to be accepted lightly.

Secondly, the so-called solutions of the paradoxes have always left me in a certain state of uneasiness. True, the theories of types have, if tenable, shown how the paradoxes can be avoided, but they have not shown how they could arise. A quite satisfactory solution of them should, in my opinion, not only consist in directions as to how to avoid them, but should also expose the errors which cause them, *i.e.* it should not only prevent, but also explain them.

I think I can give an explanation which will serve as a means of preventing them, and which is at the same time much simpler than the theories commonly used for this purpose.

Let me start by a very elementary and well-known example of a logical paradox. The expression "This sentence is false" is generally considered paradoxical because it leads to contradictory conclusions: If it is true, then it is false; and if it is false, then it is true. Since these consequences are contradictory, the expression mentioned is considered meaningless. But it is likewise thought that the expression can obtain a meaning, if the so-called "systemic ambiguity" of the words "true" and "false" is taken into account. This I think is a mistake. The expression "This sentence is false" is not meaningless because it leads to contradictory consequences, but for a much deeper and simpler reason; and it cannot obtain a meaning by means of any logical theories or devices whatever.

My point is, that the expression "This sentence is false" is not a sentence at all in any logical sense. If the expression mentioned is considered a sentence and the word "false" is considered the logical predicate of this sentence, then the logical subject cannot be the whole expression, but at most either the words "This sentence" or the designation of these words. In the first case the whole expression is meaningless because "false" is not the kind of predicate which can be meaningfully ascribed to descriptions as "This sentence". And in the latter case the whole expression is meaningless because the description "This sentence" has no object, there being no sentence to which these words can refer. In neither case can any conclusions be drawn from the expression mentioned, and no paradoxes emerge.

If the whole expression "This sentence is false" is assumed truly to be a sentence affirming its own falseness, then it has no predicate, but the expression as a whole is functioning as the logical subject of a new sentence that must read: "The sentence 'This sentence is false' is false". Then the included sentence is true, and the including sentence false. And that is the end of it. No paradox emerges.

The included expression is meaningless, however, if it is considered to affirm its own falseness. This is seen when we remember that "false" is a predicate of sentences (or propositions). When it is predicated it, therefore, presupposes that there is a sentence of which it can be predicated (wrongly or truly). But if we are merely confronted with the expression "This sentence is false", then there is no sentence of which "false" can be predicated.

The same applies, of course, to the expression "This sentence is true" which is also meaningless, although nobody has, to my knowledge, drawn any paradoxical consequences from it. And the same is, of course, the case with the expression "This sentence consists of six words", which is neither true nor false, but simply meaningless, because there is no sentence to which it can refer. When the expression "This sentence consists of six words" is often considered a true sentence the reason is, in my opinion, that we are accustomed to conceive the description "This sentence" as a description of a sentence, and the expression mentioned being a sentence in a *grammatical*, although not in a *logical*, sense, we conceive this expression as the very sentence spoken about. But then our conception is really not expressed in the grammatical sentence mentioned, but must be formulated in the more complicated sentence: "The (grammatical) sentence 'This sentence consists of six words' consists of six words", where the included expression is the logical subject and the last occurring phrase "consists of six words" is the logical predicate.

It seems to me that this analysis also explains the paradox from which we started. In the expression "This sentence is false" the description "This sentence" is rightly assumed to refer to a sentence, and no other (grammatical) sentence than the expression mentioned being presented, it is wrongly assumed that the description refers to this very expression, which then loses its logical predicate and therefore is no logical sentence at all. Not observing this change because we seem to continue to see a sentence before us, we think that we can draw contradictory consequences from it and therefore believe that we get a paradox.

The most important generalization that can be made from these considerations is, to my mind, that no sentence can refer to itself, or that no sentences are self-referring. And this can, as far as I can see, be extended to any linguistic expressions whatever, nay, to all so-called reflexive phenomena. My reasons for believing this are as follows :

A phenomenon of whatever kind it may be is not a symbol, *i.e.* is not *functioning* as a symbol, unless it is contained in a process of symbolization as a part of this process taking place in an organism. The linguistic sounds or figures are in themselves or isolated from a process of symbolization not symbols, but solely sensational phenomena produced by symbolizing human beings for whom they may represent or refer to something different from themselves. Outside the symbolizing process they are but *remnants* of a linguistic process—just as a dead organism is but the remnants of a living organism. The sounds or figures are becoming symbols again, only if they are again used as such by becoming part of a new symbolizing process. A sentential expression as such, *i.e.* as an auditive or a visual phenomenon is not by itself referring to anything, but a *human being* can use it for referring to something different from the sentential expression. He cannot, however, by means of a sentential expression refer to this very sentential expression—this fact being a consequence of the very nature of symbolization. And in the same way a word can only function as a word, if it is part of a symbolizing process. If a word or a sentential expression is to function as a linguistic expression there must be something different from them to which they may refer *via* an organism, but they can never refer to themselves—that would be tantamount to their referring to nothing. Therefore, linguistic phenomena are never self-referring or reflexive.

And this applies, I think, to all other phenomena as well. There are no reflexive phenomena at all. This seems to me to be a simple consequence of the notion of relation. Any relation presupposes at least two terms which may be more or less alike in various respects, but which can never coalesce into a single term, if the relation shall not disappear. If only a single term is given there cannot be any question of a relation. The sole one I could think of is *identity* which is often considered a relation which an object has to itself. But identity in this sense is in my opinion no relation at all. It is but another name for object—the *same* object. To be sure, we can speak of two occurrences or two appearances of one and the same object, as well as we can speak of two objects being identical in certain respects, *e.g.* when they have the *same* property or are having the *same* relation to something. That, however, does not mean that there are two properties or relations that are identical, but solely that the *same* property belongs to both objects, or that several objects are having the *same* mutual relation as some other objects have—a property or a relation being something

that cannot be localized in space or time. Two different spots of colour may have the *same* shade of colour, we say. This, however, does not mean that there are *two* identical shades of colour, but that the same (one and the same) shade of colour is to be found in the two spots of colour. There is no relation of identity between two shades of colour, but the two different *spots* of colour have a property (*viz.* the shade of colour) *in common*. The *property* is the *same*—and *this* is the sense of the phrase “the two spots of colour are identical with respect to colour”. Two different objects may have a property in common; but this fact does not constitute a relation of identity between the *objects*. As they are *two* they cannot be identical. Only the *colour* is identical in the two objects, *i.e.* they have the *same* colour, but there are not two colours with a relation of identity between them.

Further, it may be said that two different names have identical meanings, if they denote the *same* object. But it has presumably no sense to say, as it is often done, that an object is identical with itself, if “identical” should here mean (indicate) a relation. The expression “every object is identical with itself” can in my opinion solely mean, that no object is different from itself *qua* object, even if it may have various appearances in different situations. Either an object is the *same* object at various points of time or space, and if so it cannot have any relation to itself, or the object is not the same at various points of time or space, and if so there are several objects which may stand in various relations to each other, but none of which can have a relation of identity to any of the others.

If I am right in maintaining that no object or term can have any relation to itself, then all talk of so-called reflexive relations is senseless—a thesis that I would illustrate a little further by means of a few examples.

In the sentence “The blackboard is black” the word “blackboard” designates a blackboard. But in the sentence “The words ‘the blackboard’ consists of 13 letters” the words “the blackboard” designate nothing, but are the very object about which something is predicated. The last sentence really degenerates into a predication by which a predicate, *viz.* “consists of 13 letters”, is predicated about the present object, *viz.* the word-image “the blackboard”—exactly as when somebody points at a blackboard and says “is black” or solely “black”. In the sentence “The words ‘the blackboard’ consists of 13 letters” the words “the blackboard” is neither the subject-term nor the designation of the logical subject, but the very

subject itself, *i.e.* the object about which something is predicated. In this case the object, which is the *grammatical* subject of the statement, is exceptionally a *word-image*, but it is not a word, if "word" is to be understood as something that designates something.

Take then the sentence "The word 'long' is itself short". Here the word "long" designates nothing—although it is generally used to designate a *property*, which does not belong to the *word-image* "long". But in the sentence "The word 'short' is short" the second "short" designates a property that belongs to the *word-image* "short" itself. This, however, does *not* mean, that the word "short" here refers to itself. No, it designates a *property* that exceptionally belongs to the expression "short" itself, *i.e.* a property that belongs, not to the *word* "short", but to the *word-image* "short". And this *word-image* is not a word, *i.e.* it is not functioning as a symbol in a symbolizing process, but is simply an object that happens to have the property called short.

We are now able to dispose of and explain Grelling's (or Weil's) paradox according to which it is apparently possible to prove that a heterological word is not heterological. A property word (*e.g.* an adjective) is called heterological, if designating a property which does not belong to the word, *i.e.* the *word-image*, itself. The adjective "long" *e.g.* is a heterological word. If then we ask whether the word "heterological" itself is heterological, we seem to be led to contradictory answers: if it is heterological, it is not heterological; and if it is not heterological, then it is heterological. This paradox is in my opinion solved and explained when we realize that it is absurd to ask, whether the word "heterological" itself is heterological or not. The word "heterological" designates a property belonging (or not belonging) to a *word*, *viz.* a word that designates a property which belongs to the corresponding *word-image*. In order to decide whether a word is heterological or not, it is necessary to look at its *word-image*. But it has no sense to predicate the property "heterological" to the *word-image* "heterological", because the word "heterological" does not designate a property of a *word-image*, but a property of a word, namely a word designating a property. The paradox arises by a false identification of a word with its corresponding *word-image*—only the word can be meaningfully said to be heterological, whereas adjectives as "long" or "short" can only be ascribed to *word-images*. Words can neither be long or short, and *word-images* can neither be heterological nor homological.

A paradox of a similar character is Berry's paradox which runs as follows: In English, the lowest integer not nameable in fewer than 19 syllables is found to be nameable in 18 syllables. This seems to be a contradiction. The number is 111777, *i.e.* "one hundred and eleven thousand seven hundred and seventy seven". This number designation contains 19 syllables. But the number mentioned can also be unambiguously termed: "The least integer not nameable in fewer than nineteen syllables" where we have only 18 syllables.

The paradox seems to depend on the simple fact, that the relation between an object and its designation is assumed to be a one-one relation, whereas it is really a one-many relation. An object can be designated unambiguously by various different designations, and these need not contain the same number of syllables. There is no contradiction in saying: "The least integer whose number-name contains at least 19 syllables can also be named by another name that contains but 18 syllables".

Confer *e.g.* the description "the largest English town whose name contains at most two syllables" on the one hand, and "London" on the other. Here the town London is designated by the disyllable "London" as well as by the much longer description "the largest English town whose name contains at most two syllables", but there is no contradiction in naming this town in both ways. If, however, we assume that the relation of designation is one-one, then we have to face the paradox: "The largest English town whose name contains at most two syllables has a name that contains many more than two syllables". Really this statement concerns two different names, and there is no paradox at all. The appearance of paradox depends upon the wrong assumption that the town has but one name.

In a similar way it is, in my opinion, possible to dispose of and explain the paradox of "the relation which holds between R and S whenever R does not have the relation R to S"—my point being that it is meaningless to assume that a relation R can be its own relatum. But there are other paradoxes which depend on a wrong use of the word or concept of "all", and the solution of which seems to me to claim a kind of distinction of types—although a very simple one. The most famous of these paradoxes is, of course, Russell's paradox to which I will now turn.

It is well-known that this paradox concerns the class of classes which are not members of themselves. Apparently this class is a member of itself, if it is not a member of itself; and it is not a member of itself, if it is a member of itself. The solution that



Russell offers depends on the assumption that "a proposition about a class is always to be reduced to a statement about a function which defines the class, i.e. about a function which is satisfied by the members of the class and by no other arguments" (*Prin. Math.*, Vol. I, p. 62-63. 2nd edn.). Hence a class cannot, by the vicious-circle principle, significantly be an argument to its defining function, and therefore neither satisfies nor does not satisfy its defining function, and so is neither a member of itself nor not a member of itself.

While agreeing with his conclusion I am not at one with Russell in respect of the argument that leads to it. To me it seems simpler to resort to the following solution which at the same time gives an explanation of the appearance of the paradox.

A class is a collection of objects that are said to be members of the class. A class, therefore, must contain at least two members in order to be a class. Incidentally I may remark that for the same reason I consider expressions as "null-class" and "unit-class" to be merely ways of speaking (*façons de parler*) and not existing entities. If we put the particle "all" before a class name in a proposition about a class, we are not predicating something about the class as such but about its membership distributively, i.e. we are ascribing the predicate to each member of the class. The proposition "All S are P" says that each S severally has the property P—not that the class S has this property. "All horses are four-legged" means e.g. that each horse has four legs—not that the class of horses has four legs. This is the simple reason why a class is typically different from its members, and why a class cannot be a member of itself. Whatever other properties a class may have, it must contain at least two members in order to be a class at all. And a class cannot, no more than any other object, have any relation to itself, e.g. be a member of itself. That would mean that there could be a class without members, but that is absurd. Therefore, the expression "the class of classes which are not members of themselves" is a meaningless expression as well as the expression "the class of classes which are members of themselves". And therefore the paradox does not arise at all.

To be more explicit: A class, S, presupposes that there are objects which are members of it. If these objects have a common property, P, then the class S can be defined as the collection of all the objects that have this property. But this property P cannot be a property of the class S because that would mean, that S should be one of the objects belonging to the class S,



since this class was defined as the collection of *all* the objects having the property P. If we try to escape this conclusion by defining: "The class S consists of all objects having the property P *except* the class S of these objects—plus this class", then we do not know what S means and therefore do not know what we should except from this class and then add to the other objects in the class in order to get S. This way of escape is therefore impossible. To be sure, we may try to define the class S "intensionally" without knowing whether there are members in it or not according to the scheme: "The class S consists of all objects that have the property P". But such definition is to my mind analytic or verbal. It solely means that *if* there are objects having the property P we will call them members of the class S. By this we have defined the use of the word "S" but not a class, because we do not know whether such class exists or not, *i.e.* whether there are objects having the property P or not, and if there are no such objects there is no class to be defined. The intensional definition, therefore, is not a definition of a *class* but solely of a *word*, a hypothetical class name. To sum up: We must distinguish between classes and class names. Classes must at least have two members, and can never be members of themselves. So-called intensional definitions of classes are really but definitions of class names and concerns solely our language, but not real collections of objects.

At this point it will, however, perhaps be expedient to remark that the word "all" and "class" have what Russell calls "systemic ambiguity". As already mentioned the proposition "All S are P" means that every single S has P (or belongs to the class P). The collection or class of P's may be said to be an entity of another logical type than the members of it. This type may be called type I. If several classes of type I are defined, we may collect them in a super-class and say that all classes of type I are members of the super-class which is, however, of another logical type that may be called type II. And so on, in accordance with Russell's simple theory of types.

The objects from which we start the construction of this hierarchy of classes may be of various kinds. They may, *e.g.* be individual things, or single properties of individual things, or relations between individual things. But if we speak of "a property of a property" or "a relation between relations" we are moving up in a hierarchy of properties or a hierarchy of relations—each new type being characterized by its members having members of the next lower type. The word "all" also changes its designative meaning or function as we ascend the

hierarchy, the word "all" placed before a class-name in a proposition referring always to the *members* of the class named and never to that class itself.

I think there is no need of any more complicated theory of logical types. But what about the so-called reflexibility of languages?

As is well-known we can, in a certain sense, speak of a language *in* the language itself, and Carnap has even, in his "Logical Syntax of Language", taken the trouble to construct an artificial language containing its own syntax. May such languages then not be reflexive phenomena?

I don't think so, and shall now try to show why. To do so it will, I believe, be sufficient to consider our everyday language. This is, as Tarski somewhere remarks, an "open" language in the sense that its vocabulary as well as its syntax may be changed or extended as the language develops. But it is in my opinion "open" in an even more fundamental sense: If I pronounce a sentence  $S_2$  about another sentence  $S_1$ , then  $S_2$  is a new linguistic phenomenon—even if it is perhaps similar to  $S_1$  in such a way that there is but a numerical difference between  $S_1$  and  $S_2$ . If  $S_2$  consists of the "same" words as  $S_1$ , and if these words are arranged in the "same" order as these words are arranged in  $S_1$ , then  $S_2$  may be said to have spoken about  $S_1$  without changing either the vocabulary or the syntax of the language in which the two sentences are spoken. But the vocabulary and the syntax of a language is but an abstraction, a structure, while the language itself is a concrete phenomenon in time or space—or more strictly: an unlimited course or lapse of concrete phenomena. As long as a language is used, it is "open", even if its new sentences are built of elements of one and the same vocabulary, and according to one and the same syntax. Therefore a sentence can never say anything about itself. It is, indeed, not a sentence before it is finished, and if something is to be said about it, then this something must be said after the sentence is finished. The case is similar to Royce's map of England which he thought should contain a map of the map, and a map of the map of the map, and so on, in order to be complete. That is not the case. There should not be any map of a map before the first map is designed. And when the first map is designed there should only be a map of this map. In order to be complete a map of England shall never contain a (complete) map of itself. An infinite number of maps of maps of England never exists, and therefore should never be mapped, but any number of really designed maps of maps *can* be mapped,

and the latest of them is always the correct and complete map corresponding to the prevailing state of affairs—until it is finished itself. Then a new map should be made containing the preceding one. And when that one is finished a new one, and so on. As long as England and the mapping of England is not finished new maps ought to be made, but the infinity is always a progressive one and never contains a finished infinity. In a similar way the language is always progressing. Any repetition of a sentence is a new occurrence of the sentence, and a sentence cannot say anything of a *sentence* that is not yet finished, but at most of the abstract *form* which the unfinished sentence may be assumed to have—when it is finished. In the same way a language cannot say anything about this language as a whole, because the pronouncement about the “whole” language will add something to it. But one part of a language may very well say something about another (concrete or abstract) part of the language, provided that this other part already exists. And the parts may very well be similar, but can never be identical. This is but another way to say that a sentence can never refer to itself, never be its own subject.

Similarly, self-consciousness is never a reflexive phenomenon. The act of consciousness in which we are conscious of ourselves being conscious of something is always a *new* act of consciousness, and this new act of consciousness can only be conscious in yet another new act of consciousness. Therefore an act of consciousness can never be conscious of itself. But we, as persons existing in time can successively be conscious of our preceding acts of consciousness, although we can never be conscious of our present act of consciousness before it is completed. That would mean to jump over our own shadow, or rather: that would mean to be conscious of something without there being anything to be conscious of. “Introspection is always retrospection”, as Professor Ryle has said, I think.

The main point is, in my opinion, that knowing is a temporal process, and that any act of knowing must exist before we can know it—otherwise we could know or speak about an act of knowing that does not yet exist in the sense that it would be nothing at all.

In the same way we can, I believe, dispose of other seemingly reflexive phenomena as, e.g. “a theory of theories”, “a concept of concepts”, “knowledge of knowledge”, and so on. But what about the so-called reflexive classes of numbers? Are they not genuinely reflexive? I don’t think so, and I believe the so-called paradoxes of the infinite series of numbers can be

shown to depend on a failure to distinguish between two simple forms of natural numbers. But this distinction being a bit more complicated than the above mentioned I will leave it for another occasion. What I have intended here is merely to show the possibility of eliminating some of the paradoxes which many logicians nowadays seem to have accustomed themselves to in such a degree that they almost consider such anomalies as established matters of course. As far as my memory serves me, Lord Russell somewhere says that "paradoxes are the experiments of logic". I don't agree. To my mind paradoxes are rather "traps of logic", and I don't like to see logicians trapped—not even in their own traps. I, therefore, have tried to destroy some of these traps. Whether I have succeeded or not I leave to you to decide. But should my present paper merely serve as a warning against the traps, and as a reminder of being cautious, I should consider even such modest effect as not being quite unsatisfactory.

*University of Copenhagen*

## II.—ORDINARY LANGUAGE AND COMMON SENSE

By A. D. WOZZLEY

THAT philosophy has for its task the study of linguistic usage is a view so widely held these days that it has almost reached the canonical status of a platitude. We learn all that, as philosophers, we want to learn about promises, for example, by taking the word 'promise', and examining on the one hand its syntax (e.g. the contrast between 'promising to' and 'promising that') and on the other hand its semantics (e.g. the contrasts between 'I promise' and 'I promised'; between 'I promise' and 'He promises'; between 'I promise' and 'I intend'; etc.). It has been argued by Malcolm in more than one place that Moore's great achievement in philosophy has been to bring out the absurdity of philosophically sceptical paradoxes, and to bring it out by showing how they violate ordinary language. This, it is said, is what Moore was clearly doing in his "Defence of Common Sense" and again in his "Proof of the External World". To be sure, what Moore *said* he was doing was defending common sense. But Malcolm asserts that common sense and ordinary language are the same thing. His chief criticism of Moore appears to be that having successfully shown how the sceptics are violating ordinary usage by doubting or denying that there are material objects, that this is a hand, and so on, he proceeds to violate ordinary usage himself in saying 'I *know* that this is a hand'. "Moore's assertions", he says, "do not belong to 'common sense', i.e. to ordinary language, at all. They involve a use of 'know' which is a radical departure from ordinary usage."<sup>1</sup>

Some elucidation is here necessary of the phrase 'ordinary language'.

1. Malcolm has a definition of it (in "Moore and Ordinary Language"<sup>2</sup>), such that an expression is an ordinary expression if it would be used to describe a given situation. It is not necessarily the case that it is used to describe the situation, for the situation may never, or not often, occur. And an expression describes a situation (i) if it describes it correctly, or (ii) if,

<sup>1</sup> *Defending Common Sense*, Phil. Rev., 1949, p. 219.

<sup>2</sup> *The Philosophy of G. E. Moore*, ed. Schilpp, pp. 345-368.

although it describes it incorrectly, the mistake is an empirical one, which can be empirically put right. *E.g.* 'The earth is flat' is an ordinary expression.

2. It seems doubtful that Malcolm has consistently stuck to that definition. For example, in *Defending Common Sense* he uses phrases like 'natural thing to say', where he seems to be meaning that an expression is ordinary if it is the usual one employed (or a usual one to be employed) to describe a given situation whenever the situation is described (which might be very seldom). In this use 'describe' occurs as what has been called a task-word; and this sense of 'ordinary' leaves the question open whether the description is correct, and whether, if false, it is only empirically false.

In this sense of 'ordinary' 'ordinary' is much more ordinary than in Malcolm's other sense; and it is in this sense that I am using it. It is important to see the difference between the two senses. For in sense 1, the proposition which is the backbone of Malcolm's argument for Moore against the sceptical philosophers in "Moore and Ordinary Language", *viz.* that no ordinary expression can be self-contradictory, is analytic. But in sense 2, the proposition that no ordinary expression can be self-contradictory, so far from being analytic, could easily be (and maybe is) false.

The questions I want to consider are whether common sense and ordinary language are to be identified as Malcolm suggests, and what is the cogency of philosophical appeal to ordinary language. The answers I shall offer are :

1. that there is a difference between common sense and ordinary language such that you cannot argue from the absurdity of violating the latter to absurdity in disagreeing with the former.

2. that reasons can be given why their identification looks plausible—if you do not look too hard.

3. That the ascriptive, or referential, function of statements is not unlike the use of names; and that this is the way in which it is difficult for an expression, if used as it is ordinarily used, to be wrong.

First the point needs to be made that the notion of ordinary language is far from simple; and consequently that departure from ordinary usage is not to be simply determined. This can be illustrated, I think, by what Malcolm has to say about doubt in his *Defending Common Sense*. He wishes to distinguish the philosophical doubt which he thinks Moore's opponents express whether this is a hand, that is a tree, etc., from doubt whether

this is a hand, that is a tree, etc. Not merely does Malcolm want to make this distinction, but he goes on to make two further claims:

1. That in circumstances in which a man could have a philosophical doubt, he could not have a doubt—i.e. having a doubt and not having a doubt would be equally out of order.

2. That to call a philosophical doubt a doubt is as misleading as to call a rhetorical question a question.

In other words, the philosopher who says he doubts that this is a hand is simply misusing language. The reason given why he is misusing language is the familiar one that what would stop anyone else from saying that he doubts this is a hand—being able to touch it, feel its texture, cut it and see blood flow, etc.—would not stop the philosopher from saying that he doubts it is a hand.

Now consider this. When a philosopher says 'I doubt this is a hand', what does he mean?

1. He may mean by 'doubt' something different from what is ordinarily meant; and therefore his statement may not, after all, be incompatible with some other statement, e.g. 'I know this is a hand', which would appear to be a contrary of it.

That is what Malcolm thinks the philosopher means.

2. He may mean by 'doubt' the same as what is ordinarily meant. And in that case, when he says 'I doubt this is a hand' (I assume he is not lying),

(a) His statement may be false—he may not doubt it at all, although he thinks he does, or

(b) His statement may be true. What would be a satisfactory criterion of his doubting in a situation in which other people do not? Summarily, if his behaviour in the situation were relevantly different from theirs, we would probably allow that he did doubt. But, supposing his behaviour were *not* different, should we conclude that he was wrong—that he did not doubt? Not necessarily, for whereas what other people might (but in this case do not) suspect is that what we see is a plaster hand, or a stuffed hand, or a trick done with mirrors, he may suspect it of being a bundle of ideas, or a family of sense-data or a retinal image, etc. You may say that that is an odd thing to suspect, and that it can only be suspected through philosophical confusions. But it *has* been suspected.

In this situation we may say that the philosopher's doubt is different from other people's doubt, because what would confirm or resolve it is quite different from what would confirm or

resolve other people's. But we may not, on that account, say that the meaning of 'doubt' as used by the philosopher is different from its meaning as used by other people; there is no reason, so far, why they should not be just the same; and there is no reason, so far, why in one respect at least—*viz.* meaning the same by 'doubt'—the philosopher should not be conforming with ordinary usage.

This point could be brought out by comparing the philosopher's use of 'doubt' and the ordinary man's use of 'doubt' with Descartes' two uses of 'dubitare'. It is plausible to say that he did philosophically mean by 'dubitare' something different from what he meant non-philosophically. For his philosophical doubting was something which he could decide to do, as opposed to non-philosophical doubting, which it is hardly appropriate to describe oneself as doing at all, let alone deciding to do. His philosophical doubt was thus a refusal to assent to, to answer Yes to, certain questions until certain conditions were fulfilled—a policy of behaving as if, or pretending that, every proposition was or might be false, until the sufficient conditions of its acceptability were satisfied, when it was to be exempted from the doubt. With regard to this doubt it would have been inappropriate to ask Descartes, when he was practising it: "You say you doubt *p*. But do you really feel doubtful or uncertain of *p*?" It could be that, as a result of his philosophically doubting *p*, Descartes should come genuinely to feel uncertain or doubtful of *p*. But his doubting *p* philosophically and his feeling doubtful of *p* would still have been different. The difference could be summarised by saying that his policy of philosophical doubt was the policy of behaving with regard to the propositions in question as if he felt doubtful of or even disbelieved them.

Now, if Malcolm's remarks on the distinction between the philosophers' doubt and ordinary doubt had been aimed only at Descartes, he would have had a good case. But they clearly were not. He talks of philosophers (in the plural), and he repeatedly refers to "Moore's philosophical opponent", without anywhere suggesting that the phrase uniquely describes Descartes. But it is not at all clear that all philosophers who have said that they doubted propositions of the kind mentioned either said they were adopting a Cartesian policy or were adopting it. What is the evidence that they were not just in the position which, as I suggested, Descartes *could* have come to through sticking to his policy, *viz.* the position of doubting (*i.e.* feeling doubtful of) *p*? If it is true that theirs is an unusual or



extraordinary doubt, it is not on that account true that it is a mistake (or misleading) to call it doubt; nor is it on that account true that they are using 'doubt' in a different sense from other people.

Take another of Malcolm's arguments, this time a criticism of Moore himself for violating ordinary language. Moore said, as he held up his right hand, 'I know this is a hand'. But Malcolm replies that this too is a philosophers' violation of ordinary language, because Moore is saying 'I know . . .' in a situation in which, for non-philosophers, there is no question at all whether it is a hand or not. But statements of the form 'I know (that) . . .' have a use only in situations where three conditions are fulfilled:

- (i) there is some question or doubt to be settled;
- (ii) the person making the statement 'I know that . . .' has some reason in favour of it, or can prove it;
- (iii) there is some investigation which, if carried out, would settle the question at issue.

In Moore's case none of these conditions is fulfilled. Therefore in saying 'I know this is a hand' Moore is violating ordinary language. It is a case where a question of knowledge does not arise: it is equally wrong for Moore to say that he does know and that he does not know. He is perfectly entitled to say 'This is a hand', but not to say 'I know this is a hand'.

Now I do not accept Malcolm's argument either in this case of 'know' or in the earlier case of 'doubt', because I think we need to make some distinctions with regard to ordinary usage which it seems to me that he fails to make. If we take a statement which is syntactically correct and ask whether its use in a particular situation was that of ordinary language there are at least three things we may be asking, such that if the answer to any one of them is Yes, then it is wrong or highly misleading to say, if the answer to any one of the others is No, that the user is violating ordinary language, or to do what Malcolm does—to infer from the answer to one being No that the user is violating ordinary language. The three things are quite familiar:

- (a) Was the speaker using the expression with the same meaning (= sense, intension, connotation) as it is ordinarily used?
- (b) Was the speaker in using the expression referring to or calling attention to an object or situation of a kind which is ordinarily referred to in the case of that expression?
- (c) Was the speaker using the expression in circumstances in which it is ordinarily used?

Malcolm, in effect, treats the philosopher who doubts whether this is a hand as falling foul of question (a) because 'doubt' in his statement does not mean what it ordinarily means. Even if that were true Malcolm should go on to consider questions (b) and (c)—where his argument would seem more plausible in the case of the whole sentence 'I doubt this is a hand' than in the case of the word 'doubt' alone. But, as I argued earlier, he has not shown that 'doubt' is being used in a non-ordinary sense; so he has not shown ordinary language to be violated.

Moore, in claiming to know that this is a hand is criticised, in effect, by Malcolm for using 'know' in circumstances in which it would not ordinarily be used, *i.e.* falling foul of (c) alone. To say that I know this is a hand is a singularly pointless thing to say, if nobody has ever suggested that it was not, or has asked what it was, etc. But if every expression, which is pointless because it is unnecessary is to count as a violation of ordinary language, then a great deal of ordinary language is a violation of ordinary language.

What I have tried to maintain so far is that for somebody to have violated ordinary language he needs at least to have offended against (a) above—*i.e.* he needs to have used the expression in question in a non-ordinary sense. Malcolm failed to show that the sceptics were using 'doubt' in a non-ordinary sense, and made no attempt to show that Moore was using 'know' in a non-ordinary sense. To use an expression in circumstances in which it would not ordinarily be used—because it is pointless, or rude, or hurtful—does not constitute a mis-use of language which has philosophical consequences.

Now for the difference between ordinary language and common sense, which I do not find very easy to bring out, but which I think to be there. It is, of course, true that one can think of many statements in which 'common sense' occurs for which we could not sensibly substitute 'ordinary language'—*e.g.* "It's simply a matter of pure common sense", "Where's your common sense?" "Common sense tells you that the thinner the ice, the more dangerous it is to skate". But I do not think this technique of the substitution-game is any use here, because it might be, and I suspect it is, that the notion of common sense is one of a different and more abstract level than the notion of ordinary language—which might still leave it the case that they were not essentially different from each other.

Nevertheless I think they are different. If you are doing what Moore said he was doing, *i.e.* defending common sense, you are defending *beliefs*, which to be beliefs of common sense

do not necessarily have to be formulated. We all of us believe any number of things which few of us have formulated. Now the beliefs of common sense can be divided into (a) those at a matter-of-fact level: this is a chair, as opposed to a table—a golf ball, as opposed to a rackets ball—a flesh and blood hand, as opposed to a plaster hand, and so on. (b) Those at a metaphysical level: this is an independently existing object, as opposed to a group of God's ideas—I see things as they are, as opposed to a version of things altered by a complicated physiological process, and so on.

One could be wrong about beliefs of either kind. What Moore said was that we could be right, and could be obviously right. I am not quite sure which kind of belief he was talking about: it would seem natural now to say the second; but I am not sure that the distinction between the two kinds had been clearly made by the sceptics or by Moore in his attack on them.

To be defending common sense then is to be defending *beliefs*, no matter who holds them, no matter whether they have been formulated—although, for a particular belief to be defended, it must, of course, have been formulated. To be defending ordinary language, on the other hand, is to be defending what people *say* (or *would say*) in given circumstances. By 'what they say' I mean not 'the view which they express' (this *would* identify common sense and ordinary language), but 'the way they say what they say' 'the language they use to say what they say'. Now against what kind of charge can one defend ordinary language? You defend a belief of common sense against the charge of being false, or insufficiently substantiated. But if you take what is agreed to be an expression of ordinary language, what is the charge against it to be? Not that it is false, but that it is out of place here—*e.g.* because, although it is ordinarily used, it is not ordinarily used to apply to a situation of this kind, or because, although it is ordinarily used to apply to a situation of this kind, it is not ordinarily used to apply to a situation of this kind in circumstances of this kind.

The contrast between ordinary language and common sense can perhaps be brought out in this way, by contrasting the statement

(i) This (is what) is called a hand,

and the statement

(ii) This is a hand.

(i) is a statement *about* ordinary language, about the way an expression in it is used. (ii) is not a statement about ordinary

language at all, but an example of ordinary language, and might serve as the expression of a common sense belief. But we would *consider* it differently, according as we were asking a question about ordinary language or about common sense. In the first case, we are asking whether it is the (a) correct expression to use here, and corresponding to statement (i) we have the question "Is this (what is) called a hand?" In the second case, we are not asking a linguistic question at all, but a question corresponding to statement (ii) "Is this a hand?"

To know whether this is called a hand I need not know much about hands, indeed in principle I need not know anything about them at all beyond the semantic fact that people do (would) call this a hand. To know whether it is a hand I do need to know something about hands, not much perhaps but something. In the example I have used both questions might fairly be called empirical, but if we change the example and substitute 'independently existing object', then (i) "Is this (what is) called an independently existing object?" is still an empirical question but (ii) "Is this an independently existing object?" may not be an empirical question—at least if we mean by an empirical question one which is to be answered by further looking, listening, etc. A given statement then may at once be an ordinary language statement and a common sense statement, and, as the latter, may be either empirical or not; but its being an ordinary language statement is different from its being a common sense statement. Whether Moore has been doing what he said he was doing, defending common sense, or what Malcolm first said he was doing, defending ordinary language, he has not been doing what Malcolm second said he was doing, defending both because they are the same thing, because they are not the same thing.

Although the question of the truth of a common sense belief is different from the question whether ordinary language would describe a given situation as such and such, yet it can be explained why they tend not to be distinguished.

(i) A simple, if unexciting reason—that if a common sense belief is to be expressed, it must be expressed in some language or other; and it is natural enough to express it in ordinary language. Hence, for many purposes it is unnecessary to distinguish between statements in ordinary language and statements about ordinary language. This is somewhat analogous to the usefulness of a dictionary for providing non-linguistic knowledge and of an encyclopaedia for providing linguistic knowledge.

(ii) A rather more interesting reason is to be found by attending to the referential or ascriptive function of language; and it is to that that I now want to turn. A great many of the statements which make up our everyday, non-professional conversation contain proper names, and even more contain definite descriptions. Now definite descriptions are interesting because of their hybrid nature, the dual function which they perform in the statements in which they occur:

(a) Like proper names but unlike general terms they are used to individuate or designate.

(b) Like general terms but unlike proper names they have a meaning (= sense, intension). I can understand a given definite description, prior to having pointed out to me the object so described (if any is). I cannot understand a proper name in this way, for there is nothing about a proper name, simply regarded as a proper name, to be understood. Or, what comes to the same thing, one can often from hearing a definite description tell which of a group of persons or things it designates; but one cannot do that with proper names. (Nicknames like 'Ginger', 'Shorty', etc. occur as a cross between proper names and definite descriptions.)

Now, I may use a definite description wrongly in either of its functions. It may mis-describe the object in question, or it may mis-designate it—*i.e.* it may not be an expression employed to designate that particular object at all. But whereas the descriptive error, if there is one, is not corrigible by appeal to usage, the ascriptive error is so corrigible. At Thame a one-day agricultural show is held each September, and for years it has been billed as "England's Largest One-Day Show". Maybe it is, maybe it is not—for the Yeovil Agricultural Show is also billed as England's Largest One-Day Show. But, as long as the Thame Show is called that, then it is called that; and there is a respect in which if I designate it by that expression I cannot be wrong; and if I designate some other show by that expression I can be shown to be wrong by an appeal to usage. But an appeal to usage will not determine whether Thame's is the largest one-day show; and it is perhaps fortunate either for Thame or for Yeovil that they are a hundred or more miles apart.

No fun fair is nowadays complete without its sideshow called 'The Wall of Death', but its proprietor could not stay in business for long, if it were a wall of death. But if at the fair you asked the way to the Wall of Death you would feel swindled by

the purist who tried to tell you there could not be one there, but misdirected by the man who sent you to the Dodgems.

Even general words, even when not functioning as parts of definite descriptions, may have in statements in which they occur a referential function. If I complain that the battery on my car is flat, and I am right, then there is a property of the battery referred to by the word 'flat'. I do not want to carry this too far, or to say anything so absurd as that all words in all statements are ascriptive, but I do want to suggest that we should not underrate the ascriptive aspect of words, in particular of definite descriptions.

And if we pass to complete sentences, we find that we have all kinds of more or less standard expressions, where, because they are almost ritual expressions, the descriptive element is at a minimum—not totally absent, for we sometimes want to disagree with such a statement. If, for example, it is raining fairly hard outside, and a man comes in and says "It's pouring with rain" we would not ordinarily bother to distinguish that from "It's raining", "It's wet outside", "Miserable weather again", "Filthy night", and so on. We know what he is referring to, and we let it go at that, for any of these expressions is accepted as referring to a situation of that kind. But we might, if we wanted to be a nuisance, take him up on the ground that he was exaggerating, or that it is not raining as hard as it was last night, when he called it only a shower.

So, if I use a more or less standard expression to call attention to things and situations to which it would ordinarily be used to call attention, it is natural to say I must be right. And this is what Malcolm in his article on "Moore and Ordinary Language" was arguing. But if my argument is sound he has oversimplified the matter. If I do use an expression the way it is ordinarily used, then I must be right ascriptively—but I may be well wrong descriptively. There is nothing wrong with a schoolboy saying, as I believe they do now, that he has been given a supersonic pen-knife for Christmas, as long as he does not believe a false proposition about the velocity of his knife, any more than there is anything wrong with an astronomer continuing to refer to  $\alpha$  Tauri as a first magnitude star, and  $\beta$  Pegasi as a second magnitude star even after it has been established that the former is much smaller than the latter.

What I am wanting to suggest is :

(i) That a comparison between the expressions of ordinary language and names is worth making. For this helps to explain why we think that in some way or other the state-

ments of ordinary language are the best there are and cannot be wrong.

(ii) That we need to distinguish the way they cannot be wrong from the way they can be wrong. I am not at all sure that Moore did this, and consequently I am not at all sure that, although he said he was defending common sense, and although he said he was proving the existence of an external world by holding up his right hand, he was not really making use of the fact that if I use an expression according to its ordinary usage I cannot be misusing it as if it were the fact that if I use an expression according to its ordinary usage what I say is surely true. I have tried to argue that the first fact is not the last, because the last is not a fact at all, and because, if it were, it would still be different.

When introduced to somebody for the first time one is told his name. It would be absurd later to say to him "I know everybody calls you John Jackson, and I do not suspect you of using the name as an alias, but are you really John Jackson?" or "I know your name is 'John Jackson', but are you really John Jackson?" When introduced to an object for the first time we may equally well be told "This is a so-and-so" or "This is what we call a so-and-so". When in my early days in the army I was first introduced to the Bren gun, the sergeant instructor's patter was wholly in this form: "This is what we call the Bren gun". Not merely that, which might be claimed as very similar to "His name is 'John Jackson'", but also "This is what we call a gas-operated machine gun . . . what we call the breech block—the gas vent—the piston post—the flash eliminator—etc." In this case, and this is where the difference between a description and a proper name comes in, it would have been sensible, although not perhaps for a recruit, to ask "I know that's what you call the flash eliminator, but is it a flash eliminator?" It would not have been sensible for the recruit to ask that question, partly because it is not sensible for recruits to speak out of turn, and partly because the sergeant probably would not have seen the difference. We do not for ordinary purposes readily distinguish or need to distinguish "This is a . . ." from "This is what we call a . . ." But for philosophical purposes they do need to be distinguished. So, whereas "This is a hand", so far as it is being used to refer to what "This is a hand" is ordinarily used to refer to, *i.e.* so far as it is equivalent to "This is what we call a hand" cannot (analytically) be wrong, "This is a hand", so far as it is being used to tell us that this is a hand, can be wrong; or if it cannot,

we need some other reason why it cannot than the appeal to ordinary language. What looks to be an appeal to ordinary language is really an appeal to common sense in disguise.

Our use of expressions is governed both by ascriptive and by syntactical rules, with the consequence that there is no reason in principle why an expression should not always be false, or inaccurate, or even self-contradictory, and nevertheless the correct expression to use. But, in fact, because syntactical rules tend to be determined by (and so to be changed, if necessary, to adapt themselves to) ascriptive rules, expressions which may at one time be ascriptively correct but descriptively false may change their meanings so as to fit their ascriptive rules. It is arguable that, whether or not 'artificial silk' once meant artificial silk, it does not mean that now. The slide from literal to metaphorical meaning is somewhat similar (*e.g.* Malta—the unsinkable aircraft carrier).

Therefore Malcolm while wrong in saying that an ordinary expression cannot always be false or self-contradictory, should rather have said that it is unlikely always to be so, because the expression tends to change its meaning in such a way that it achieves the result of being true if correct.

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### III.—INFERENCE AND MEANING

BY WILFRID SELLARS

#### I

TWENTY or so years ago it was received dogma among the great majority of empirically-minded philosophers that the inference which finds its expression in "It is raining, therefore the streets will be wet" is an enthymeme. Explicitly formulated, it was claimed, the argument thus presented would read, "Whenever it rains the streets will be wet, it is raining; therefore the streets will be wet". As the validity of this reasoning rests on purely formal principles, it was concluded that the same is true of the briefer argument above, it being in all respects save formulation, identically the same. Thus, when Metaphysicus rehearsed for their benefit the argument "I am releasing a piece of chalk, therefore *of necessity* it will fall", adding by way of commentary, "Surely that was a reasonable argument. It is not, however, formally valid, so the necessity in question cannot be logical necessity. Must you not, therefore, admit that the inference is based on an appeal to a non-logical or material necessity?" our empiricists replied with the above analysis, and dismissed the subject with the remark, "It is now obvious that the only necessity involved is the logical necessity with which 'This chalk will fall' follows from 'All released pieces of chalk fall' and 'This piece of chalk is being released'."

One need not be persuaded by this retort to feel its force. After all, are there not such things as enthymemes? and is not the rephrased argument valid on purely logical grounds? Convincing though the retort may be, however, it scarcely amounts to a disproof of the idea that there are *material* as well as *formal* principles of inference, so that instead of merely being abridged edition of a formally valid argument, "It is raining, therefore the streets will be wet" might well be as it stands a valid argument, though warranted by a *material* principle of inference. On what grounds would our empirically minded philosophers have rejected this idea? At least a partial answer lies close at hand. A scrutiny of the above clash with Metaphysicus suggests that tacit use is being made of Ockham's razor. The claim seems to be that even if it made sense to speak of non-logical principles of inference, there would be no need for them. For do not logical principles enable us to do all the arguing

and inferring which these supposed material principles could warrant, provided we use the generalizations which correspond to these material principles as *premises* in our arguments? Thus, if we suppose "*x is an acid*" may be inferred from *x turns litmus paper red*" to be a material principle of inference, the corresponding generalization would be " $(x) x \text{ turns litmus paper red} \supset x \text{ is an acid}$ ". The material rule would certify the argument, "This turns litmus paper red, therefore it is an acid", while if we use the generalization corresponding to the rule as a premise, we get the logically valid argument, " $(x) x \text{ turns litmus paper red} \supset x \text{ is an acid}$ ; this turns litmus paper red; therefore this is an acid".

I think it is clear, however, that our empirically-minded friends would have gone much farther than this. They would have attacked the very notion of a material principle of inference. At the very least they would have claimed that if any principles do correspond to this description, they have a thoroughly second-rate and/or derivative status as compared with purely formal principles. We can imagine that something like the following considerations would have governed their thinking on this matter.

'Formal rules of inference are essential to the very possibility of language; indeed, of thought. Kant was on the right track when he insisted that just as concepts are essentially (and not accidentally) items which can occur in judgments, so judgments (and, therefore, indirectly concepts) are essentially (and not accidentally) items which can occur in reasonings or arguments. Without formal rules of inference there would be no terms, no concepts, no language, no thought. In this sense, our empiricists continue, one could say that logical rules of inference specify, at least partially, the very form of a term or concept. Were it not for these rules, we could not even conceive of the releasing or the falling of a piece of chalk, not to mention the piece of chalk itself. On the other hand, given these rules and given the course of our sense-experience, no other rules of inference (that is, no non-formal or material rules) are necessary conditions of concepts—though rules of inductive inference may be necessary to establish synthetic truths involving them.'

To bolster up this line of thought, they would appeal to the empiricist account of concept formation in one or other of the various forms in which it has been held, since Locke made it the cornerstone of his philosophy, and continue:

'The *form* of our concepts may depend on rules of inference, but their material *content* does not. Even if we were to acknowledge a material rule of inference whereby "This piece of chalk

will fall" can legitimately be inferred from "This piece of chalk is being released", the rule could have nothing to do with our ability to conceive of either *chalk*, the *releasing of chalk*, or the *falling of chalk*. This fact alone would force us to put material principles of inference, should we acknowledge their existence, on a decidedly inferior plane.'

Can one, however, go this far in cutting material rules of inference down to size, without taking the more drastic step of denying that anything is really described by the phrase "material rule of inference"? Those who take this line claim that "It is raining, therefore the streets will be wet", when it isn't an enthymematic abridgment of a formally valid argument, is merely the manifestation of a *tendency to expect* to see wet streets when one finds it raining, a tendency which has been hammered into the speaker by past experience. In this latter case it is the manifestation of a process which at best can only *simulate* inference, since it is an habitual transition of the imagination, and as such is not governed by a principle or rule by reference to which it can be characterized as valid or invalid. That Hume dignified the activation of an association with the phrase "causal inference" is but a minor flaw, they continue, in an otherwise brilliant analysis. It should, however, be immediately pointed out that before one has a right to say that what Hume calls "causal inference" really isn't inference at all, but a mere habitual transition from one thought to another, one must pay the price of showing just how *logical* inference is something more than a mere habitual transition of the imagination. Empiricists in the Humean tradition have rarely paid this price, a fact which has proved most unfortunate for the following reason. An examination of the history of the subject shows that those who have held that "causal inference" only simulates inference proper have been led to do so as a result of the conviction that *if it were genuine inference, the laws of nature would be discovered to us by pure reason*. But an adequate account of *logical* inference might make it clear that even "causal inference" can be genuine inference, as it seems to be, without this unwelcome consequence.

A somewhat less drastic approach to material rules of inference differs from the above in admitting that there are such rules, and that they are indeed rules of *inference*, but insists that not only do they have second-class status in that, unlike formal rules, they are not necessary conditions of the very existence of terms or concepts, but also that their authority as rules is purely *derivative*. It claims that recognition of a material rule to the effect that "x is B" may be inferred from "x is A" presupposes

prior acceptance of what we have called the corresponding generalization, in this case "All A is B", and owes its authority to the fact that " $\bar{x}$  is B" is logically derivable from " $x$  is A" together with "All A is B". Those who adopt this alternative concede to Metaphysicus that the inference from "It is raining" to "The streets will be wet" is immediately grounded in a material rather than formal rule of inference, but insist that as the authority of material principles is purely derivative, this admission entails none of the rationalistic consequences which he desiderates. While they might agree with proponents of a more drastic approach that in some cases utterances and inscriptions of "It is raining, therefore the streets will be wet" are functioning merely as abbreviated expressions of inferences governed by a formal rule of inference, they are more likely to insist (and I believe correctly) that in most cases, at least, these supposed abridgments of formally valid arguments are actually complete arguments as they stand which are validated by material rules of inference. They would add that it might not be inappropriate to say that these arguments are "abridgments" or "enthymemes" provided that these terms are taken to imply not that there are no material rules of inference, but rather that their status is purely derivative, and their contribution to thought a matter of convenience.

If neither of these two more drastic lines is taken, it would seem possible (at least at this early stage of our discussion) to take a different tack and combine the ascription of an inferior status to material rules of inference, as not being necessary conditions of the existence of terms or concepts, with the claim that their authority as rules is nevertheless original. This view in turn, would seem to admit of two variants. According to the first, material principles of inference, though not essential to meaning, are as indispensable as formal rules to thought about empirical matters. The second variant denies this, claiming that although the authority of material rules is not inherited from formal rules, but is equally original, they are nevertheless dispensable modes of thought, making no contribution to its penetration or scope which could not be duplicated by a combination of formal rules and factual premises.

Now, all the above possibilities in the way of empirically minded interpretations of material rules of inference have in common the idea that whereas formal rules are necessary conditions of the existence of concepts or the possession of meaning by terms, and, in this sense, are generic conditions of meaning—the specific content of a concept, or meaning of a term, is derived

from experience, and is prior to any material rules of inference in which this concept or term may come to play a role. But might it not be possible for an empiricist to hold that material rules of inference are as essential to meaning as formal rules? that the specific nature of a factual concept is determined by the material rules of inference governing it, as its generic nature is determined by formal rules of inference? that the meaning of a term lies in the materially and formally valid inferences it makes possible? In spite of the fact that a position of this kind is incompatible with the so-called "empiricist" theory of concept formation, and is universally relegated to the absolute idealisms and rationalisms of a bygone age, I mention it for the sake of completeness.

In effect, then, we have been led to distinguish the following six conceptions of the status of material rules of inference:

- (1) Material rules are as *essential to meaning* (and hence to language and thought) as formal rules, contributing the architectural detail of its structure within the flying buttresses of logical form.
- (2) While not essential to meaning, material rules of inference have an *original authority* not derived from formal rules, and play an *indispensable* role in our thinking on matters of fact.
- (3) Same as (2) save that the acknowledgment of material rules of inference is held to be a *dispensable* feature of thought, at best a matter of convenience.
- (4) Material rules of inference have a *purely derivative authority*, though they are genuinely rules of inference.
- (5) The sentences which raise these puzzles about material rules of inference are *merely abridged formulations of logically valid inferences*. (Clearly the distinction between an inference and the formulation of an inference would have to be explored.)
- (6) Trains of thought which are said to be governed by "material rules of inference" are actually *not inferences at all*, but rather activated associations which mimic inference, concealing their intellectual nudity with stolen "therefores".

## II

In the above paragraphs we have been led to worry about the dispensability or indispensability of, and the relation to meaning of, material rules of inference. We have not yet, however,

given an account of what a material rule of inference is, or pretends to be. We have relied on dangerously vague historical connotations of the terms "formal" and "logical", as well as on the use of examples. Fortunately, help lies close at hand. Professor Rudolf Carnap, in his *Logical Syntax of Language*, draws a systematic contrast between two types of syntactical rule which if his syntactical conception of logic is sound, are exactly the formal and material rules of inference with which we are concerned. It is to a brief exposition of his views on this matter that I now turn.

In Carnap's terminology, a rule of inference, conceived to be a syntactical rule, is called a "transformation rule". He emphasizes the central role played by the concept of a transformation rule in the definition of a language. Indeed (p. 168) he contends that once we know the circumstances under which one expression of a language is the direct consequence of another, we have the key to the logical structure of the language. These circumstances are specified by the transformation rules, which are formulated in the syntactical metalanguage of the language to which they apply. Whether stated as rules of inference, or as a definition of "direct consequence in S",

... all that is necessary is that it be clear to what forms of expression the rules are in general applicable (which gives us the definition of "sentence" and under what conditions a transformation or inference is permitted (which gives us the definition of "direct consequence") (p. 170).

Transformation rules must carefully be distinguished from *valid* sentences in the object language. The latter are sentences which require nothing more than an appeal to the transformation rules of the language to justify their assertion. If an object-language sentence is valid, its contradictory is *contra-valid*. If either valid or contra-valid, it is said to be *determinate*, otherwise *indeterminate*. Carnap finds it to be a distinguishing feature of logical symbols and expressions that each sentence constructed solely from them is determinate (p. 177). On page 175 he defines the *content* of a sentence as the class of non-valid sentences which are its consequences (i.e. can be inferred from it).

We next note that Carnap draws a distinction between *logical* and *extra-logical* transformation rules. The essential difference, to put the matter in a way which is adequate for our purposes, is that whereas *logically* valid inferences do not, *extra-logically* valid inferences do depend for their validity on the fact that they contain a certain set of descriptive terms. The syllogism so fatal to Socrates remains valid if any three descriptive terms of

appropriate category are systematically substituted for "men", "mortal" and "Socrates". In Quine's useful terminology, descriptive terms occur *vacuously* in logically valid arguments; *essentially* in extra-logically valid arguments. Now, the most obvious candidates for the position of extra-logical rule of inference are rules authorizing inferences which, to be *logically* valid would have to have as an additional premise a sentence formulating a law of nature. Carnap calls rules of this kind "P-rules" (Where the "P" is short for "physical" in a suitably broad sense), as contrasted with L-rules (logical rules). In his terminology, therefore, he distinguished between L-valid and P-valid inferences. To illustrate: If we suppose " $(x) \phi x$ " to state a law of nature,

I.  $(x) \phi x \supset \psi a$ , but  $\phi a$ , therefore  $\psi a$   
would be an L-valid inference.

II.  $\phi a$ , therefore  $\psi a$   
would be a P-valid inference. The P-rule authorizing it, whatever its most satisfactory formulation might turn out to be, would be to the effect that "A sentence consisting of ' $\psi$ ' followed by an individual constant is validly inferred from a sentence consisting of ' $\phi$ ' followed by that same individual constant". (That we cannot rest in this formulation is shown by the fact that when the phrase "may be inferred from" is correctly used in ordinary speech, it is preceded and followed not by the names of sentences, but by the sentences themselves—e.g. that it will rain can be inferred from the darkness of the clouds.)

Corresponding to this distinction between L-valid and P-valid inferences, we have the distinction between L-valid and P-valid sentences. Thus,

III.  $(x) \phi x \supset \psi x \cdot \& \cdot \phi a : \supset \cdot \psi a$   
would be an L-valid sentence. On the other hand, given the above P-rule,

IV.  $\phi a \supset \psi a$   
would be a P-valid sentence, while

V.  $\phi a \& - \psi a$   
would be P-contravalid.

Furthermore, in view of Carnap's definition of the content of a sentence as the class of the non-valid sentences which are consequences of it, ' $\psi a$ ' would be part of the content of ' $\phi a$ ', though not of its L-content. Given a suitable definition of the content of expressions other than sentences, a corresponding



distinction would have to be drawn between the content of an expression governed by P-rules, and its content in the narrower sense of L-content.

Let us now raise the question whether, granted that a language must have rules of inference, it must have both L-rules and P-rules. We might expect Carnap to say that whereas a language without descriptive terms need not, and, indeed, cannot have other than logical rules of inference, a language with descriptive (extra-logical) terms must have extra-logical rules. *Carnap, however, makes it clear that in his opinion a language containing descriptive terms need not be governed by extra-logical transformation rules.* Indeed, he commits himself (p. 180) to the view that for every language with P-rules, a language with L-rules only can be constructed in which everything sayable in the former can be said. If we now turn back to our list of six possible accounts of the status of material rules of inference (above, p. 317), we see at once that Carnap's account falls in neither the first nor the second category for according to these, P-rules would be indispensable. Furthermore, since he clearly holds that P-rules are as genuinely rules of inference as are L-rules, it does not belong in the fifth or sixth category. Assuming the adequacy of our classification, we are left with the third and fourth pigeon-holes in which to place his account.

To be sure, Carnap, in the above passage, is not discussing the syntax of natural languages, but rather the construction by logicians of artificial languages. Yet he is clearly conceiving of these artificial languages as candidates for adoption by language users. And presumably, an artificially constructed calculus with an appropriate syntactical structure, becomes a natural language by virtue of (1) the adoption of its syntactical rules by a language speaking community ; (2) the association of certain of its descriptive terms with sensory cues. Thus, in saying that "whether in the construction of a language S, we formulate only L-rules, or include also P-rules . . . is a question of expedience", Carnap is implying that natural languages need have no P-rules, and that the presence or absence of P-rules in a natural language is a matter of some form of (presumably unconscious) social selection determined by convenience.

Notice that corresponding descriptive terms in two languages, one with and one without P-rules, though they have the same meaning in the sense that they enable the communication of the same information, need not have the same content, in Carnap's syntactical sense of the term. For the content of a term ' $\phi$ ' is, roughly speaking, the totality of what is entailed *logically* or



*physically* by the function " $\phi x$ ", and, clearly, a term governed by P-rules will have a greater content than one which is not.

Now, according to the fourth alternative, P-rules are not only dispensable, but have a purely derivative authority. Concretely this amounts to the suggestion that the authority of P-rules derives from the fact that the inferences they certify can be reformulated as logically valid inferences, if the generalizations which have been canonized into P-rules are brought down to earth as additional premises. The contribution made by P-rules would then be one of convenience only, and they would be of little interest to the philosopher. They would permit us to argue " $\phi a$  therefore  $\psi a$ " provided we accepted the generalization " $(x) \phi x \supset \psi x$ " and could, therefore, argue " $(x) \phi x \supset \psi x \ \& \ \phi a$ , therefore  $\psi a$ ", a saving, perhaps, of some intellectual breath at the level of argument, but one which brings no basic enrichment to the language. Now, Carnap nowhere commits himself—at least in so many words—to this fourth conception of the status of P-rules. Might it not be the case that his views fall into the third category? Perhaps we can find him to hold that although dispensable, and adding nothing to the factual content that can be communicated by the language, P-rules enable a language to perform a function which could not be duplicated (even at the cost of great inconvenience) by a language with L-rules alone. If there were any evidence to this effect, we might attribute to him the view that at least part of the authority of P-rules, even though what it authorizes is dispensable, is not derivative from that of L-rules. However, when one turns to Carnap's book with these questions in mind, one is startled to find no account whatsoever of the grounds on which it might be expedient to adopt a language governed by P-rules as well as L-rules. What we do find is an emphasis on the disadvantage of adopting P-rules. He points out that to the extent that empirical generalizations are erected into P-rules, science is put into a strait-jacket. "If P-rules are stated, we may frequently be placed in the position of having to alter the language" (p. 180). Now, although the phrase "alter the language" is perhaps a bit drastic for the adding or subtracting of P-rules conceived as conveniences with purely derivative authority, there is nothing here which prohibits us from construing Carnap as holding that when the adoption of P-rules is expedient, it is merely because at that time and in those circumstances, the economy in the number of premises required for inferences which is obtained by building scientific generalizations into the very machinery of the language, more than compensates for the resulting tendency of this machinery to impede

scientific progress. In any event, the passage from which we have just quoted contains no hint that the expediency of adopting P-rules rests on their ability to authorize something that would not be authorized in a language with L-rules alone.

At this point it is relevant to mention that according to Carnap, P-rules, like L-rules, may take either one of two forms : (1) They may be formulated as rules of inference. This is the form we have supposed them to have in the above discussion. (2) They may be formulated as sentences to the effect that certain sentences in the object language are "primitive sentences", that is, *privileged* sentences in that their assertion is unconditionally authorized by the rules of the language. Notice, however, that each form may be established on the basis of the other provided that the language contains, as it must, at least one L-rule of the first form, i.e. formulated as a rule of inference, in short a rule of detachment or *modus ponens*. It is interesting, however, to note that although P-rules may be introduced in either form, Carnap prefers to state them in the second form as singling out certain object-language sentences (usually generalized material implications) to be primitive sentences. This inevitably suggests he is not thinking of the expediency of the adoption of P-rules as a matter of diminishing of the number of premises needed for inferences. For when P-rules are stated in the second form, the generalizations they characterize as primitive sentences must be used as premises in inferences, even though as being unconditionally assertable on the authority of the P-rules of the language, they are premises of a privileged kind.

### III

Now, we may well imagine Metaphysicus to have been following the above exploration of Carnap's views with the most intense interest. He has read with approval Carnap's account of the formal distinction between L-rules and P-rules of inference, but shared our disappointment at Carnap's failure to explain either the status or the specific contribution of the latter. Metaphysicus notes that we have been asking whether Carnap's P-rules authorize any linguistic activity which, dispensable or not, is incapable of being authorized by L-rules alone. Pointing out that we have as yet failed to find any mention of such in the *Logical Syntax of Language*, he now seizes the initiative with the claim that there is indeed such an activity, and that it provides the key to an understanding of the status of material rules of inference.

What Metaphysicus has in mind, of course, are such subjunctive conditionals as "If I *had released* this piece of chalk, it *would have fallen*", and "If there *were to be* a flash of lightning, there *would be* thunder". But before Metaphysicus attempts an analysis of these statements, bringing out their relevance to our problem, he first turns his attention to those subjunctive conditionals which are clearly true on purely formal grounds. He points out that "If anything were red and square, it would be red" cannot plausibly be claimed to assert the same as "(In point of fact) all red and square things are red", and suggests that this subjunctive conditional conveys the same information as the logical rule permitting the inference of *x is red* from *x is red and x is square*. This rule is a derivative logical rule, a special case of the logical rule proper, which latter, of course, does not single out the terms *red* and *square*. According to this line of thought, one who asserts "If this *were* red and square, then it *would be* red", is committing himself to the falsity of "This is red and square", while in some sense giving expression to a logical rule of inference. On the other hand, a person who says "Since this is both red and square, it is red", is giving expression to the same rule of inference, while asserting both "This is red and square", and "This is red". Metaphysicus now argues that if we accept this analysis, we must interpret the subjunctive conditionals with which we began this paragraph as expressions of *material* rules of inference. "If there were to be a flash of lightning, there would be thunder", giving expression to some such rule as "*There is thunder at time t-plus-n* may be inferred from *there is lightning at time t*", and this rule is not in any obvious way a specification of a purely logical rule of inference. He therefore claims to have shown beyond reasonable doubt not only that there are such things as material rules of inference, but, which is far more important, *that they are essential to any conceptual frame which permits the formulation of such subjunctive conditionals as do not give expression to logical principles of inference*. Since we are all conscious of the key role played in the sciences, both formal and empirical, in detective work and in the ordinary course of living by subjunctive conditionals, this claim, if substantiated, would indeed give a distinguished status to material rules of inference.

At this point, our empiricists are tempted to reply by claiming that even the latter subjunctive conditionals owe their force to purely logical principles and that if this does not appear to be the case it is because the content of these conditionals has not been made fully explicit. This is, of course, essentially the same

claim as the one considered at the opening of this paper to the effect that "It is raining, therefore the streets will be wet", is an enthymeme. It will prove quite rewarding, however, to explore this claim in its present guise.

What, then, would be the explicit formulation of this subjunctive conditional? Perhaps,

- A. *Since every time it rains the streets are wet*, if it were to rain the streets would be wet,

the since clause dropping out to give the usual formulation. The logical principle of inference sanctioning this expanded version would presumably be "From ' $(x) \phi x$  implies  $\psi x$ ' can be inferred ' $\phi a$  implies  $\psi a$ '", which is a special case of the principle authorizing the inference from " $(x) \phi x$ " to " $\phi a$ ". But we see right away that something is wrong. For the subjunctive conditionals which this principle authorizes would be of the form "If  $(x) \phi x$  were the case, then  $\phi a$  would be the case". Consequently, if "Every time it rains the streets are wet" expresses a material implication, as it must, if we are not to introduce a P-rule in the very attempt to dispense with such, we would get a subjunctive conditional of the form "If it were the case that  $(x) \phi x \supset \psi x$ , then it would be the case that  $\phi a \supset \psi a$ ". But the "since" statement corresponding to this is " $(x) \phi x \supset \psi x, \phi a \supset \psi a$ ". In other words, the logical principle would justify not A, but rather

- A<sup>1</sup>. *Since every time it rains the streets in point of fact are wet*, it will rain  $\supset$  the streets will be wet.

Here the subjunctive mood has disappeared from the consequence clause, and with a merely material implication, we are no longer asserting that a wetting of the streets can be inferred from the occurrence of rain. Nor is it an adequate reply that "it will rain  $\supset$  the streets will be wet" is inferable from "all cases of rain are in point of fact cases of wet streets", and that it is this inferability which makes its presence felt in the original subjunctive conditional. For on this alternative, wherever we accept "all A's are in point of fact B" we should be warranted in asserting "if  $x$  were A,  $x$  would be B"—whereas whenever we assert a subjunctive conditional of the latter form, we would deny that it was merely in point of fact that all A's are B.

On the other hand, if "Everytime it rains the streets are wet" is interpreted as the expression of an entailment, then the above-mentioned logical principle of inference would warrant a subjunctive conditional of the form "If it were the case that  $(x) \phi x$

entails  $\phi x$  then it would be the case that  $\phi a$  entails  $\phi a$ ". The corresponding "since" statement would be "Since  $(x) \phi x$  entails  $\phi x$ ,  $\phi a$  entails  $\phi a$ ". Thus we would get,

A'. Since *every time it rains the streets are wet* (interpreted now as an entailment), *it will rain entails the streets will be wet*.

Since an entailment statement has the same force as a subjunctive conditional, A' is equivalent to A, and our logical principle of inference has given us what we want. But a moment's reflexion reminds us that to get A' we have had to pay the price of introducing a material rule of inference. *To say that rain entails wet streets is to convey exactly the same information as to say that a sentence asserting the existence of wet streets may be inferred from a sentence asserting the existence of rain*. Thus our ultimate purpose of explaining the original subjunctive conditional without appealing to a material rule of inference would not have been achieved.

Let us try again. Perhaps the explicit formulation would be,

B. If it were the case both that *everytime it rains, the streets are wet* and that *it is raining*, then *the streets would be wet*.

The logical principle which finds expression in this statement is, schematically, "From ' $(x) (\phi x$  implies  $\psi x)$  and  $\phi a$ ' can be inferred ' $\psi a$ '". Notice that on this interpretation the original subjunctive conditional would not be the implicit formulation of a since sentence, as the since clause would include the assertion of "It is raining", and this would be incompatible with the significance of contrary to fact subjunctive conditionals. Now it is at first sight not too implausible that the original subjunctive conditional is an abbreviated formulation of B. But to see that this won't do it is sufficient to point out that on this interpretation *all such subjunctive conditionals would be true!* Surely some sentences of the form "If  $a$  were  $\phi$ ,  $a$  would be  $\psi$ " are false, in other words some sentences of the form; "Even though  $a$  were  $\phi$ , it need not be  $\psi$ " are true. But on the theory under examination, the former, when explicated turns out to be a logical truth, and the latter a contradiction.

Now, unless some other way can be found of interpreting such subjunctive conditionals in terms of logical principles of inference, we have established not only that they are the expression of material rules of inference, but that the authority of these rules is not derivative from formal rules. In other words, we have shown that material rules of inference are essential to the language we speak, for we make constant use of subjunctive conditionals of

the type we have been examining. It is very tempting to conclude that material rules of inference are essential to languages containing descriptive terms. Yet to draw this conclusion would be hasty, for the most we have shown is that if there are descriptive languages which are not governed by material rules, they do not permit the formulation of material subjunctive conditionals. We now notice that, as a matter of fact, most of the linguistic structures Carnap considers, being extensional, do not even permit the formulation of subjunctive conditionals, and that though they are not natural languages in actual use, he clearly thinks that they could be. Carnap, then, is clearly convinced that subjunctive conditionals are dispensable.

Does this commit him to holding that P-rules are dispensable? Clearly not, no more than it follows from the dispensability of logically true subjunctive conditionals that a language need have no L-rules. Thus, even though material subjunctive conditionals may be dispensable, permitting the object language to be extensional, it may nevertheless be the case that the *function* performed in natural languages by material subjunctive conditionals is indispensable, so that if it is not performed in the object language by subjunctive conditionals, it must be performed by giving direct expression to material rules of inferences in the meta-language. *In other words, where the object language does not permit us to say "If  $a$  were  $\phi$ , it would be  $\psi$ " we can achieve the same purpose by saying " $\psi a$ " may be inferred from ' $\phi a$ '".* Since it is the importance of the function served by material subjunctive conditionals on which we have been insisting, the fact that Carnap emphasizes the possibility of extensional descriptive objective languages by no means rules out the idea that material rules of inference might be indispensable to languages containing descriptive terms.

To sum up the results of the last few paragraphs: Alternative (4) has been shown, at least provisionally, to be untenable. This would leave Carnap with alternative (3)—(material rules of inference are dispensable but underived). However, in the process of disproving alternative (4) we have been led to notice the importance of the function played in natural languages by material subjunctive conditionals. Since these are object language expressions of material rules of inference, and since the same function can be performed by the formulation of a rule of inference in the metalanguage, it has occurred to us that alternative (2)—material rules of inference, though not essential to the meaning of descriptive terms, are indispensable features of languages containing descriptive terms, and have an authority

underived from formal rules—though rejected by Carnap, is worth reconsidering.

Now, if we were to accept the second alternative, it is clear that we should have to explore the relation of material rules of inference to the meaning of descriptive terms, to see if we could rest in alternative (2) without ultimately embracing alternative (1)—according to which material rules of inference are as essential as formal rules to the meaning of descriptive terms. It is also worth noting, at this stage, that the Humean suggestion that causal inferences are really not inferences at all, but rather habitual expectations masquerading as inferences, loses all plausibility when it is stretched to cover ostensible material subjunctive conditionals, particularly when contrary to fact. Yet if we are now in a position to insist that materially valid inferences are as much inferences as formally valid inferences, we must also recognize that we have as yet given no account of what a rule of inference is (whether formal *or* material). It is to this task that we now turn, in the hope of getting further light on our problem.

#### IV

We have already had occasion to remark on the central role played in Carnap's conception of a language by the notion of a rule of inference or "transformation rule". Indeed, he writes on occasion (*e.g.*, p. 4) as though a language, formally considered, were identical with its syntactical rules, from which it would follow that the transformation rules of a language would be at least a part, and might—in the light of the passage we have quoted on the power of transformation rules to specify the syntactical structure of a language—be identical with the language. Now, I think we would all grant that there is a sense in which a calculus, or a game (*e.g.* chess) or even a language, is what it is as specified by certain rules. But surely there is a perplexing Hibernian ring to the statement that a calculus is identical with "its" rules. After all, the rules of a calculus belong in the syntactical metalanguage, so that in making this identification, one would be identifying a calculus with expressions in its metalanguage, and thus doing violence to a distinction which is the central theme of Carnap's book. Let me hasten to add that the identification of a calculus, or game or language, with its rules, though strictly a mistake, can be regarded as a paradoxical way of stating an important truth; and I have dwelt on the matter only because Carnap's statement is



symptomatic of a carelessness with the term "rule" which pervades his otherwise admirably incisive and patiently meticulous argument.

Another *prima facie* puzzling feature of Carnap's treatment of transformation rules is his preference for formulating them as definitions of "direct consequence in S", where S is the language whose rules are under consideration. Thus, in a passage already quoted, Carnap writes, "In the following discussion we assume that the transformation rules of any language S, i.e. the definition of the term 'direct consequence in S', are given" (p. 168). Now, this term, like any other syntactical predicate, is for Carnap, a formal predicate. That is to say it is to be defined in terms of structural properties of the expressions belonging to language S. Thus, by telling us that transformation rules can be formulated as definitions of "direct consequence in S", Carnap gives the impression that the force of a rule to the effect that expressions of kind A can be "transformed" into expressions of kind B, relates solely to the existence of a structural relationship between these two kinds of expression. In ethics the corresponding thesis would be that moral rules can be formulated as definitions, in naturalistic terms, of the predicate "morally right"; thus, the rule "Happiness ought to be maximized" as the definition " $x$  is morally right  $=_{\text{D}} x$  maximizes human happiness". Here we should all know what to say. We would point out that the definiendum is no mere synonym for the definiens, and that even if it has the same components of descriptive meaning as the latter, it has a surplus meaning over and above these which can be indicated by the word "ought". In other words, the most that such a definiens can do is specify the type of circumstances in which a certain kind of action ought to be done; it cannot specify *that* it ought to be done. If one is an emotivist in one's account of *ought*, one will say that the "cognitive content" of a rule is indeed exhausted by the definiens in such a definition; and that provided one does not overlook the surplus pragmatic meaning of the definiendum, there need be nothing mistaken about the enterprise of formulating moral rules as naturalistic definitions of "morally right". Intuitionists, on the other hand, would hold, of course, that such definitions are in principle mistaken.

Now, the basic moral of the above discussion is that if a definition is, with any plausibility, to do the work of a rule, the definiendum must have the normative flavour characteristic of "ought", or "ought not" or "may" or "may not". But when one turns to Carnap's thesis that transformation rules



may be formulated as definitions of "direct consequence in S", one finds no such flavour. The term "direct consequence" has the same sort of feel as "next to" or "between". This is not true of the predicate i.e. "derivable" in terms of which he formulates certain transformation rules which are more restricted in scope than those he associates with the predicate "direct consequence". The term "derivable" is one of those "-able" words which connotes "may be done" in the sense *not* of "can be done" but rather "is permissible", an expression which obviously belongs in the context of rules. Now it is my impression that when Carnap was looking for another word to share the burden of transformation rules formulated as definitions with "directly derivable", he failed to bear in mind that what he needed was another word with this same rulish force. If he could not find one in current use, it would have been better to make one up (e.g. "directly extractable") than to choose a word with such purely cognitive flavour.

The next point I wish to make is the closely related one that a rule is always a rule for *doing* something. In other words, any sentence which is to be the formulation of a rule must mention a doing or action. It is the performance of this action (in specified circumstances) which is enjoined by the rule, and which carries the flavour of *ought*. With this in mind, let us examine Carnap's formulation of certain transformation rules as definitions of "directly derivable in S". Here the interesting thing to note is that while the definiendum seems clearly to mention a kind of action, namely, deriving something directly from something else, and to indicate that this deriving is permissible, the definiens on the other hand, specifies only a structural relationship between the *terminus a quo* and the *terminus ad quem* of the deriving. In short, Carnap's claim that he is giving a definition of "directly derivable in S" is a snare and a delusion. It is as though one offered the following "definition" as a formulation of a basic rule governing the activities of policemen: "X is arrestable =<sub>DF</sub> X has broken a law". It is obvious that such a definition would be a mistake not only because the definiendum "arrestable" has, as we saw, a normative force not shared by the definiens, but also because it designates an act, the act of arresting, which is not designated by the definiens. I think we would all be inclined to say that a person who offered such a "definition" was really attempting, in a confused way, to do something quite different, namely, specify the circumstances in which a person is arrestable. "X is arrestable if and only if X has broken a law" reminds us of "X is a triangle if and only

if  $X$  is a plane figure bounded by three straight lines", an analytic statement which is true by definition. In both cases an "if and only if" sentence is affirmed which is not an empirical assertion. Yet it would be a mistake in principle to take " $X$  is arrestable if and only if  $X$  has broken a law" to be an analytic proposition which is true by definition. Compare, "I will shoot you if and only if you cross that line". In short, instead of defining "directly derivable in  $S$ " Carnap is at best specifying the circumstances in which it is permissible to derive one expression from another. The same considerations apply *mutatis mutandis* to Carnap's formulation of less restricted transformation rules as definitions of "direct consequence in  $S$ ". As the technical difference between the more and less restricted transformation rules considered by Carnap is irrelevant to our problem, and as we have found the term "derivable" to be more satisfactory than "consequence" we shall use the former in a broad sense which covers the ground of Carnap's two terms "derivable" and "consequence".

What, then does it mean to say of one sentence,  $B$ , that it is derivable from another,  $A$ ? Roughly, that it is permissible to assert  $B$ , given that one has asserted  $A$ , whereas it is not permissible to assert not- $B$ , given that one has asserted  $A$ . In other words, we have here a rule of conditional assertion (which must not be confused with a rule for the assertion of a conditional). To be contrasted with rules of this type, *e.g. modus ponens*, are rules which specify certain sentences as unconditionally assertable. Rules of this latter type are formulated by Carnap (with all the mistakes criticized above) as definitions of "primitive sentence of  $S$ ". Thus, to say that " $(x) \phi x \supset \psi x$ " is a primitive sentence of  $S$ , is to say that one is authorized by the rule of  $S$  to assert this sentence, *without having to appeal to evidence or grounds, in other words, to other sentences on whose prior assertion the authorization would depend*. It should, of course, be noticed that to say that a sentence is unconditionally assertable entails that its contradictory ought not to be asserted. In this respect an unconditionally assertable sentence differs from a contingently assertable sentence, *e.g.* "It is raining", whose contradictory is also contingently assertable.

Let us now pause to sum up the substance of the last few paragraphs. We have been pointing out that a syntactical rule, like any other rule, prescribes or permits a certain kind of action in a certain type of circumstance. In the case of syntactical rules, the relevant kind of action would seem to be *asserting*, a concept of which we have offered no analysis, but

which is, we shall assume, to be understood in terms of the concept of a token, so that to assert a sentence is to bring about the existence of a token of that sentence. (Though after Ryle's painstaking analysis of mentalistic terms we must be prepared to find that even the "event" of asserting has a dispositional component.) Be this as it may, it follows from our analysis that a syntactical metalanguage cannot permit the formulation of syntactical *rules*, unless (1) it contains a term for the activity of asserting, and (2) it contains an expression having the force of "ought". To the extent that a so-called "syntactical metalanguage" falls short of these requirements, it is an abstraction from a syntactical metalanguage proper. It is undoubtedly convenient to study calculi by means of such truncated metalanguages as mention only the structural inter-relationships of the sign-designs of these calculi, but it is essential for our purposes to stress that these truncated metalanguages become capable of formulating *rules* only when supplemented by the equipment mentioned above.

## V

We are now in a position to develop an account of the logical and physical modalities which, though based on Carnap's account in his *Logical Syntax of Language*, is an improvement in that it explicitly takes into account the *rulishness* of syntactical rules. It will be remembered that the central concept of Carnap's treatment is that of a quasi-syntactical sentence. As a simple example we may take the sentence "Red is a quality". This is a quasi-syntactical sentence in that it conveys the same information as the syntactical sentence "'Red' is a one-place predicate". Furthermore, "red is a quality" is a quasi-syntactical sentence in the *material* mode of speech, as opposed to the *autonomous* mode of speech, in that "'red' is a quality" is not a syntactical sentence conveying the same information as "red is a quality". Carnap tells us that

. . . The material mode of speech is a transposed mode of speech. In using it, in order to say something about a word (or a sentence) we say instead something parallel about the object designated by the word (or the fact described by the sentence. . . .)

Consider, now, the sentence "If a is red and square, then it is *logically necessary* that a be red". According to Carnap's account, this is a quasi-syntactical sentence in the material mode of speech

which conveys the same information as the syntactical sentence " 'a is red' is an L-consequence of 'a is red and a is square'." Now, as I see it, this account is essentially sound, and is vitiated only by the fact that Carnap's account of the consequence relation makes it merely a matter of a structural relationship obtaining between two expression designs. If, in accordance with our earlier proposal, we reformulate the above in terms of the syntactical predicate "derivable", then the claim becomes that the sentence "If a is red and square, then it is logically necessary that a be red" is a quasi-syntactical sentence conveying the same information as the syntactical sentence, "'a is red' is L-derivable from 'a is red and a is square'".

To appreciate the significance of this claim, let us remember our previous conclusion that in thinking of one expression as derivable from another, we are thinking of one kind of activity as permissible and of another kind as not permissible, in a certain kind of circumstance, where, for syntactical purposes, the significant feature of both activities and circumstance is that they involve the exemplification of specified types of linguistic structure. Let us now notice that the contrast between the permissible and the non-permissible can be explicated in terms of *ought to be done*, to say of x that it is permissible being to say that it is not the case that it ought not to be done. Let us assume, then, that consciousness of ought to do is the basic consciousness involved in recognizing a set of rules, whether they be moral rules or, as in the present case, rules of syntax; and that consciousness of *may do* is to be understood in terms of it.

Returning now to the problem of interpreting modal sentences, we notice that Carnap's analysis has become the claim that sentences involving the phrase "logically necessary" convey the same information (the use of the vague expression "convey the same information" is deliberate) as syntactical rules to the effect that we may do thus and so, and ought not do this and that, in the way of manipulating expressions in a language. The language of modalities is interpreted as a "transposed" language of norms.

This theory, as it stands, is open to two related and rather obvious objections. (1) It might be objected that the thought of necessity is radically different from the thought of permission-cum-obligation. (2) It might be objected that the sentence "If a is red and square, then a must, of logical necessity, be red", mentions neither linguistic expressions nor language users, and consequently cannot mention an obligation of language of language-users to use linguistic expressions in certain ways;

whereas, as we have seen, the sentence " '  $a$  is red ' is L-derivable from '  $a$  is red and  $a$  is square ' " does both.

To answer these objections, it is sufficient to remind ourselves that there are two senses in which an utterance can be said to convey information. There is the sense in which my early morning utterance, " The sky is clear ", conveys meteorological information ; and there is the sense in which it conveys information about my state of mind. Let us use the term " asserts " for the first sense of " conveys ", and " conveys " for the second. Then it is clear that if Carnap's theory is to hold water, it must be reformulated as the claim either (1) that the utterance " '  $\psi a$  ' is L-derivable from '  $\phi a$  ' " asserts what the utterance "  $\phi a$  necessitates  $\psi a$  " conveys, or (2) that the utterance " '  $\psi a$  ' is L-derivable from '  $\phi a$  ' " conveys what the utterance "  $\phi a$  necessitates  $\psi a$  " conveys.

To choose between these alternatives, it suffices to ask What does the utterance "  $\phi a$  necessitates  $\psi a$  " convey ? Clearly it conveys (and does not assert) that the speaker conforms to the rule " '  $\psi a$  ' is L-derivable from '  $\phi a$  ' ", and says what he says in some sense because of the rule. In other words, the utterance conveys the existence of a rule-governed mode of behaviour in the speaker. But it is equally clear that the utterance " '  $\psi a$  ' is L-derivable from '  $\phi a$  ' ", being a normative utterance, does not describe the psychological mechanisms of the speaker. Consequently, " '  $\psi a$  ' is L-derivable from '  $\phi a$  ' " does not assert that which is conveyed by "  $\phi a$  necessitates  $\psi a$  ", and we are left with the second of the above alternatives.

Moreover, it also follows from considerations like these that although utterances of the term " necessary " have psychological implications which overlap with those of utterances of " ought " in the context of linguistic rules, neither the term " necessary " nor the term " ought " designates a psychological property. In short, modal terms, normative terms and psychological terms are mutually irreducible. Note also that because utterances of "  $\phi a$  necessitates  $\psi a$  " convey but do not assert the existence of a linguistic rule governing the use of '  $\phi$  ' and '  $\psi$  ', there is no contradiction in the sentence "  $\phi a$  would necessitate  $\psi a$  even though there were no language users ". Opponents of the position we are developing should be wary of saying that according to it " necessities are created by linguistic rules ".

Let us now agree, and in so doing we continue in the spirit of Carnap's philosophy, that everything which can properly be called a conceptual awareness of qualities, relations, particulars or states of affairs, can be identified with the occurrence (in human

beings) of symbol-events, events of which it can correctly be said that they "mean such-and-such". Included in the class of symbol-events are events which belong to languages as social phenomena. I shall, however, for present purposes, assume that the class of symbol-events coincides with the class of linguistic events in the narrower sense. Specifically, I shall assume that concepts are meaningfully used predicates. "Necessary" and "ought", as occurring in living English usage, then, are concepts. Indeed, they would seem to be as much concepts as "red" or "longer than". Yet there is an important difference between logical, modal and normative predicates, on the one hand, and such predicates as "red" on the other. In the case of the former, it is obvious that their conceptual meaning is entirely constituted by their "logical grammar", that is, by the fact that they are used in accordance with certain syntactical rules. In the case of the latter, this is not obvious—though, as we are about to argue, it is equally true.

Why is it obvious (once we escape from the mental eye) that the conceptual meaning of a modal or normative term is constituted by its logical grammar? Because it is obvious that it cannot be constituted by the term's being a learned response to a class of extra-linguistic particulars. A modal or normative property (if we permit ourselves to speak of them as such) cannot significantly be said to be exemplified by a particular (or pair of particulars). On the other hand, it does make sense to speak of a particular as an instance of *red*, and of a pair of particulars as an instance of *longer than*. It does make sense to speak of "red" as a learned response to red objects. It would therefore seem open to us to hold that the conceptual meaning of "red" is constituted (apart from its purely formal properties) by this relationship.

Now, that at least some of the descriptive predicates of a language must be learned responses to extra-linguistic objects in order for the language to be *applied*, is obvious. But that not even these predicates ("observation predicates") owe their conceptual meaning to this association should be reasonably clear once the following considerations are taken into account:

(1) By no means all descriptive predicates which are not themselves observation predicates are explicitly definable in terms of observation predicates. The conceptual meaning of those which are not cannot consist in being learned responses to objects of the kind they are said to mean.

(2) To say of a predicate " $\phi$ " that it is an observation predicate entails that it is a learned response to extra-linguistic

situations of a certain kind  $K$ , where  $K$  is the kind of which it is correct to say " $\phi$  means  $K$ ". But, clearly, one can grant that the successful use of language requires, for certain predicates " $\phi$ ", a coincidence of the kind of object evoking the verbal response " $\phi$ " with the kind of object which " $\phi$ " is (correctly) said to mean, without identifying " $\phi$ " is evoked by  $K$ " with " $\phi$  means  $K$ ".

(3) "(In Schmidt's language) ' $rot$ ' means red" ( $S_1$ ) appears to *assert* an empirical relationship between " $rot$ " as used by Schmidt, and the class of red objects. Once this is taken for granted, it is natural to infer that this relationship consists in Schmidt's having learned to respond to red objects with " $rot$ ". If one should then notice that "(In Schmidt's language) ' $und$ ' means and" ( $S_2$ ) can scarcely be given the same interpretation, one is likely to say that  $S_2$  concerns a different species of meaning, and informs us that Schmidt uses " $und$ " in accordance with rules which are analogous to our rules for "and". Now the truth of the matter is that *neither  $S_1$  nor  $S_2$  makes an empirical assertion*, though both *convey* empirical information about Schmidt's use of language. The "means" of semantical statements (idealized as "Designates" in the Pure Semantics of Carnap and Tarski) is no more a *psychological* word than is the "ought" of ethical statements or the "must" of modal statements, even though it is correctly used, and gains application through being used, to *convey* psychological information about the use of language. And once we cease to be hypnotized by the form " $\phi$  means red" into taking for granted that the psychological fact (conceptual meaning) corresponding to  $S_1$  is a dyadic relation between Schmidt's " $rot$ " and red, and realize that since the fact in which we are interested is conveyed rather than asserted by  $S_1$ , so that the logical form of the latter is no guide to the form of the fact for which we are looking, we see that " $rot$ " might well owe its conceptual meaning to Schmidt's using " $rot$ " in accordance with rules analogous to our rules for "red".

(4) That it is fruitful to distinguish those aspects of the use of an observation predicate which relate to its *application* from those which relate to its conceptual meaning, has been obscured by a careless use of the term "rule". There is at first sight some plausibility in saying that the rules to which the expressions of a language owe their meaning are of two kinds, (a) syntactical rules, relating symbols to other symbols, and (b) semantical rules, whereby basic descriptive terms acquire extra-linguistic meaning. It takes but a moment, however, to show that this



widespread manner of speaking is radically mistaken. Obeying a rule entails recognizing that a circumstance is one to which the rule applies. If there were such a thing as a "semantical rule" by the adoption of which a descriptive term acquires meaning, it would presumably be of the form "red objects are to be responded to by the noise *red*". But to recognize the circumstances to which this rule applies, one would already have to have the concept of red, that is, a symbol of which it can correctly be said that it "means red".

(5) A uniformity in behaviour is rule-governed not *qua* uniformity, for then all habitual responses would be obeyings of rules—which is clearly not the case—but *qua* occurring, in a sense by no means easy to define, because of the conception of the norm enjoined by the rule. Yet the fact that both rule-governed and merely associative uniformities are *learned* uniformities, and differ in this respect from, say, the uniformities studied in chemistry, has blinded many philosophers to the important respects in which they differ from one another, and has led to much of the nonsense peddled under the heading "ostensive definition".

## VI

It will be remembered that at the end of section III we had arrived at the conclusion that P-rules are indispensable to any language which permits the formulation of material subjunctive conditionals, though the use of the latter may be avoided by a direct statement of the rules themselves. This, in turn, inclined us to hold that P-rules are essential to any language which contains non-logical or descriptive terms. This would eliminate all but the first two interpretations of the status of material rules of inference listed at the end of section I. If, however, the argument of section V is sound, it is the first (or "rationalistic" alternative to which we are committed. According to it, material transformation rules determine the descriptive meaning of the expressions of a language within the framework established by its logical transformation rules. In other words, where ' $\psi a$ ' is P-derivable from ' $\phi a$ ' (in modal language,  $\phi a$  necessitates  $\psi a$ ), it is as correct to say that ' $\phi a \supset \psi a$ ' is true by virtue of the meanings of ' $\phi$ ' and ' $\psi$ ', as it is to say this where ' $\psi a$ ' is L-derivable from ' $\phi a$ '. In traditional language, the "content" of concepts as well as their logical "form" is determined by rules of the Understanding. The familiar notion (Kantian in its origin, but present in various disguises in many contemporary systems) that the form of a concept is determined by 'logical rules',



while the content is 'derived from experience' embodies a radical misinterpretation of the manner in which the 'manifold of sense' contributes to the shaping of the conceptual apparatus 'applied' to the manifold in the process of cognition. The contribution does not consist in providing plums for Jack Horner. There is nothing to a conceptual apparatus that isn't determined by its rules, and there is no such thing as choosing these rules to conform with antecedently apprehended universals and connexions, for the "apprehension of universals and connexions" is already the use of a conceptual frame, and as such presupposes the rules in question. The role of the given is rather to be compared to the role of the environment in the evolution of species; though it would be misleading to say that the apparent teleology whereby men "shape their concepts to conform with reality" is as illusory as the teleology of the giraffe's lengthening neck. After all, it is characteristic of modern science to produce deliberately mutant conceptual structures with which to challenge the world. For primitive thought the analogy is much less misleading.

Our thesis, in short, turns out, as we have developed it, to be quite unlike the dogmatic rationalism of Metaphysicus. For whereas he speaks of *the* conceptual frame, *the* system of formal and material rules of inference, we recognize that there are an indefinite number of possible conceptual structures (languages) or systems of formal and material rules, each one of which can be regarded as a candidate for adoption by the animal which recognizes rules, and no one of which has an intuitable hallmark of royalty. They must compete in the market place of practice for employment by language users, and be content to be adopted haltingly and schematically. In short, we have come out with C. I. Lewis at a "pragmatic conception of the *a priori*". Indeed, my only major complaint concerning his brilliant analysis in *Mind and the World Order*, is that he speaks of the *a priori* as *analytic*, and tends to limit it to propositions involving only the more generic elements of a conceptual structure (his "categories"). As far as I can gather, Lewis uses the term "analytic" as equivalent to "depending only on the meaning of the terms involved". In this sense, of course, our *a priori* also is analytic. But this terminology is most unfortunate, since in a perfectly familiar sense of "synthetic", some *a priori* propositions (including many that Lewis recognizes) are synthetic and hence *not* analytic (in the corresponding sense of "analytic"). That Lewis does not recognize this is in part attributable to his ill-chosen terminology. It is also undoubtedly due to the fact that in

empirically-minded circles it is axiomatic that there is no synthetic *a priori*, while the very expression itself has a strong negative emotive meaning. Whether or not it is possible to rescue this expression from its unfortunate associations I do not know. I am convinced, however, that much of the current nibbling at the distinction between analytic and synthetic propositions is motivated by what I can only interpret as a desire to recognize the existence of synthetic *a priori* propositions while avoiding the contumely which the language traditionally appropriate to such a position would provoke.

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#### IV.—DISPOSITIONS AND PHENOMENALISM

By R. J. SPILSBURY

My aim in this paper is to discuss "phenomenalist" analyses of statements about dispositions, and to suggest their inadequacy. By "phenomenalist" I mean the sort of analysis which Professor Ryle gives in *The Concept of Mind*, where statements about dispositions are interpreted as elliptical formulations, the full expansion of which involves the production of a set of hypothetical propositions specifying the observable reactions of individual things and organisms to a variety of possible situations. Professor Ryle writes: "To possess a dispositional property is not to be in a particular state, or to undergo a particular change; it is to be bound or liable to be in a particular state, or to undergo a particular change, when a particular condition is realised" (p. 43). "To say that this lump of sugar is soluble is to say that it would dissolve, if submerged anywhere, at any time and in any parcel of water. To say that this sleeper knows French, is to say that if, for example, he is ever addressed in French, or shown any French newspaper, he responds pertinently in French, acts appropriately or translates it correctly into his own tongue" (p. 123). Professor Ryle's central point can I think fairly be epitomized thus: dispositions are permanent possibilities of performance. To speak of an individual's dispositions is a shorthand way of summarising a variety of observable episodes and reactions, actual and possible. To look for something specific of which a dispositional term is the name would be rather like expecting to discover some particular pool of water to correspond to the expression "the average annual rainfall of Great Britain". Jane's knowledge of French or diligence is not something over and above the individual episodes of her life and work, any more than the average annual rainfall of an area is something over and above the particular falls of rain which occur in that locality. To think of Jane's diligence as a kind of permanent *substratum* controlling the diligent deeds which Jane does or might do, is to misconstrue the "grammar" of dispositional statements. These refer, not to actual states which are unobservable, but to the possibility of observable states and actions.

To empirically-minded persons, the attractiveness of phenomenalist doctrines lies in the claim made on their behalf that

they enable us to dispense with "unobservables", or risky inferences to the unobservable, while doing full justice to the meaning of our everyday utterances about things and persons. In the prevailing philosophical climate, however, there is a strong tendency to reject this claim, so far as phenomenalist analyses of statements like "There is a table next door" are concerned. It is widely believed that the phenomenalist cure is worse than the alleged disease, in this class of cases. On the other hand, similar analyses of dispositional statements, with a distinct phenomenalist flavour, seem to have escaped these criticisms, despite the fact that they are exposed to many similar objections. The same philosophers can be found attacking the view that objects are permanent possibilities of sensation and defending the view that dispositions are permanent possibilities of performance, with little awareness of the ambivalence of their offensive-defensive operations. Professor Ryle affords the most notable example of this, who rejects the view that talking about such things as gate-posts is talking in certain ways about the *sensa* and *sensibilia* of different observers: yet his own view that talking about dispositions is talking in certain ways about observable occurrences and would-be occurrences displays quite a close affinity to the view he rejects. In his analysis of dispositional statements, *acts* and *possible acts* fill a similar role to that filled by *sensa* and *sensibilia* in phenomenalist theories of perception. I shall try to show that this is so by offering various criticisms of the Rylean treatment of dispositions, criticisms suggested for the most part by familiar objections to the doctrine traditionally known as "Phenomenalism".

One of the standard arguments against phenomenism is that no phenomenalist analysis can ever be completed. Statements about physical objects are said to imply an infinite number of hypothetical observation-statements, so that no finite series of such statements can exhaust the full significance of the statement under analysis, and a complete analysis in these terms is logically impossible. The original statement always tells us more than any phenomenalist "translation" that can be produced. Moreover, the rule for constructing the infinite series of hypothetical statements cannot be given except by reference to the concept of a physical object which is supposed to be under analysis.<sup>1</sup>

Now consider certain typical examples of the Rylean analysis of dispositional statements: "When an object is described as hard, we do not mean only that it would resist deformation;

<sup>1</sup> Cf. Kneale, *Probability and Induction*, p. 86.

we mean also that it would, for example, give out a sharp sound if struck, that it would cause us pain if we came into sharp contact with it, that resilient objects would bounce off it, and so on indefinitely. If we wished to unpack all that is conveyed in describing an animal as gregarious, we should similarly have to produce an infinite series of different hypothetical propositions" (p. 44).

Since it is logically impossible to produce an infinite series of hypothetical propositions, this reads like a *reductio ad absurdum* of this particular programme of analysis: yet Professor Ryle does not draw this conclusion. It is necessary therefore to examine his argument more closely.

The crucial part of his analysis of the meaning of the statement that an object is hard comes immediately after the last hypothetical proposition which he cites, in the words "and so on". A small phrase, this, but more compact of meaning than many pretentious expressions, which carry their importance on their faces, as it were. One of its few rivals as an important brevity is the symbol  $\infty$ . Its use in the present context seems to suggest that one could extend the list of hypothetical statements indefinitely, given time and patience; and that it is a contingent fact, without much logical interest, that one has to stop one's enumeration somewhere, and exhibit its incompleteness by this invitation to the reader to continue the analysis for himself, if he feels so inclined. But this invitation can only be accepted by one who knows the rule for the construction of this series, the rule which distinguishes *this* series of hypothetical statements from other series of a similar form. The important question is whether this rule can be defined without re-introducing the dispositional concept which is under analysis. If it cannot be, the analysis, however specious, cannot be regarded as satisfactory.

Let us test this out in one or two examples. If an object is described as hard, this means that it would resist deformation, give out a sharp sound if struck, cause us pain if we came into sharp contact with it, and . . . and what? *And behave in any of the other ways characteristic of a hard object.* There seems to be no other way of specifying the rule of this series, the principle according to which candidates for this series are selected, except by reference to the concept of *hardness*. Take another example: To say that a person is vain is to say that when he is in company he tends to talk a lot about himself, and cleaves to the society of the eminent, and rejects criticism, and seeks the footlights, *and so on: i.e. and acts in all those other ways characteristic of*

*vain persons*. Here again, the rule of the series is provided by the predicate "vain".

If this argument is sound, it means that dispositional concepts supply the indispensable rule for the construction of the series of hypothetical propositions, and cannot therefore be logically eliminated from any analysis of the kind illustrated above. But before forming a definite opinion about this, it is necessary to consider the distinction drawn by Ryle between simple, "single-track" dispositions, the actualisations of which are "nearly uniform", and many-track dispositions, the exercises of which are "indefinitely heterogeneous" (p. 44). This is the same distinction as he draws, in different terminology, between generic tendencies described by generic or determinable words, and specific tendencies described by specific or determinate words (p. 118). Most of the "higher-grade" dispositions of human beings are said to fall within the generic category.

Now the difficulties raised above seem most acute in the case of generic dispositional words. Where "simple" dispositions are concerned, such as the brittleness of glass or the smoking-habits of a man, it is possible to regard the dispositional term as a shorthand device for referring to the class of episodes, actual and possible, which constitute or would constitute the numerically-different exercises of the disposition. Since the episodes are said to be uniform, or nearly so, the hypothetical propositions which describe them form a unified series, and the rule for extending the series can be defined in terms of the mutual resemblance between the members of the series. For example, to say that a person has the smoking-habit would simply be to say that he smokes or would smoke on this, that or the other occasion. The dispositional sense of "smoke" can be defined in terms of the "episodic" sense, and if anyone were in doubt about the meaning of the expression "habitual smoker", it would be possible to point out people actually smoking and explain "It means doing that sort of thing pretty regularly". I shall maintain later that this analysis is inadequate for other reasons, but I do not think the arguments so far adduced are fatal to it.

The case seems otherwise with Ryle's analysis of generic disposition-words and statements. Here the difficulty is to discover any relationship between the various episodes, and the hypothetical propositions which describe them, to justify their being grouped together in a single set or series. I do not understand Ryle's use of the term "generic" in this context. In any ordinary use it would be absurd to say that *hardness* was a

genus, of which the properties of causing pain, resisting deformation, and giving out a sharp sound were species. There is, indeed, the verbal appearance of contradiction between Ryle's use of "generic", and his stress on the heterogeneous character of the episodes which can be subsumed under any generic dispositional term. If "heterogeneous" is meant literally, we can understand the difficulty of defining the rule for the series of hypothetical propositions, since to describe a collection as "heterogeneous" is to deny any principle of selection determining the collection. All heterogeneous collections, *qua* heterogeneous, are alike, and if one is to distinguish one such collection from another, some additional principle is required. As far as I have been able to discover, Professor Ryle has not specified what form this principle takes, and the gap here can only be filled by using the rule supplied by the dispositional concept, which thus becomes an indispensable factor in its own analysis. It is impossible in cases of this kind to define the dispositional use of a word in terms of its episodic use or uses, since Ryle denies that there is any episodic use for many words of this class (p. 118).

One possible answer to this criticism is that it ignores the distinction drawn by W. E. Johnson between class-membership and the relationship between the determinates of a common determinable. According to Johnson, the reason why we subsume red, green and yellow under the common determinable *colour* is because of a "relation of difference" between red, green and yellow, and not because of any characteristic common to them all, for there is none. But Johnson saw that it would not do to say that red, green and yellow were "merely" different from one another, for in that case there would be no special reason for grouping red, green and yellow under one determinable, and round, square and oblong under another determinable. He therefore suggested that the relation of difference between the determinates of a common determinable was a unique kind of difference: the determinates were not merely "other than" one another, but "opponent to" one another. That is to say, there is an incompatibility between the determinates of a common determinable, which we express when we say that it is impossible for an object to be characterised by more than one of these determinates at the same time. Now whatever may be thought of this doctrine of Johnson's, it seems clear that it is irrelevant to Ryle's analysis of generic dispositional terms. For in the latter case, there does not seem to be any necessary incompatibility between the specific episodes

concerned, which Ryle describes as "heterogeneous". To revert to our former example, there is no such incompatibility between causing pain, resisting deformation and giving out a sharp sound, since these properties may all be manifested by the same object at the same time. It is possible that Ryle is extending Johnson's doctrine to cover a much wider class of cases, but the logical difficulties involved in such an extension seem to me very great.

My own view is that many of the logical problems raised by Ryle's analysis can be traced to a failure to distinguish between the meaning of a statement and the evidence for its truth or falsity. There are, no doubt, a great many different ways of testing the truth of most dispositional statements, corresponding to the wide variety of evidence that is or might be relevant to the claims made by such statements. But the fact that a statement can be confirmed in lots of different ways does not imply that it means lots of different things, except on a theory of meaning now generally—and rightly—rejected. The suggestion that part of the *meaning* of "This table is hard" is that it would give out a sharp sound if struck, or that it would cause pain to anyone colliding with it, seems to me an example of this confusion. A deaf person may be unaware that hard objects emit sharp sounds when struck, without in any way failing to understand statements which ascribe hardness to objects. To say that a person is vain is not the same as saying that he tends to talk a lot about himself, and cleaves to the society of the distinguished, and seeks the footlights, etc., since no *contradiction* is involved in asserting that he does all these things and denying that he is vain. It is always a logically-open or logically-permissible question whether he does these things out of vanity, or from some other motive. It may be absurd to doubt the man's motive on the evidence of his behaviour, but this is quite different from saying that the motive-statement means the same as a conjunction of statements about behaviour. If this were true, the question whether the conjunction of behaviour-statements fully establishes the motive-statement would be logically meaningless.

Much more could be said about this, but I shall not pursue these points further for fear of digressing too far from my main theme. I must, however, point out that the diagnosis offered above, if correct, explains why Ryle's analysis of (generic) dispositional statements seems to involve a back-reference to the rule supplied by the predicates of such statements. For if the series of different hypothetical propositions describe different



ways of finding out whether a certain dispositional property is present or not, if they constitute a sample of the possible evidence which is relevant in different circumstances to the hypothesis that an individual possesses that disposition, then there is no reason why these hypothetical propositions should have anything in common except this evidential relationship to the corresponding dispositional statement which they all support. The evidence relevant to a hypothesis may be extremely varied, as every reader of detective novels knows. The only thing holding together such an assortment of evidence is the fact that it all bears on the same point, or points in the same direction. Omit this reference-point, and the assortment remains simply an assortment, with no claim to be grouped together and considered as a single class, logically distinct from other classes. Ryle's assortment of different hypothetical propositions derives its claim to be regarded as a single series, logically distinct from other series, from the fact that each of the hypothetical propositions states one of the truth-conditions of a particular dispositional statement. The members of the series are grouped together because of their common relationship to a statement *outside* the series, and therefore the rule of the series cannot logically be extracted from the series itself, merely by considering the internal relationship between the members of the series. The dispositional statement is the *raison d'être* for the series, and supplies the necessary rule for its construction. Upon this the series depends for its connectedness as a single series, distinct from other series.

Whatever validity these criticisms may possess, they are directed primarily against one special form or aspect of the phenomenalist interpretation of dispositional statements, as developed by Professor Ryle, and I do not wish to concentrate solely on this aspect. The fundamental assumption in any phenomenalist programme for the "dissolution" of dispositions is this: that statements about dispositions are statements about sets of observable acts and episodes, actual and possible, and do not refer to any unobservable states or processes which may be thought to determine these particular actions and reactions. Thus, an angry disposition is not to be regarded as a fit of temper which goes into long periods of unobserved retirement, or as an occult cause of particular outbursts of temper; to speak of an angry disposition is simply a way of speaking about actual and possible outbursts of temper. To speak about the persistence of a dispositional state, when no observable manifestation of this state is occurring, is to speak about the

possibility of their being observable activity of a specifiable kind, if certain conditions were realised. Phenomenalist theories of dispositions, like phenomenalist theories of physical objects, fill in the gaps between what is intermittently observed with a host of things which *might* be observed, and seek thus to avoid the hypothesis of unobserved states and objects existing independently of observation, without wholly abandoning the notions of permanence and continuity.

Now there is nothing about this general programme or theory which entails that statements about dispositions should be analysed in terms of a set of *heterogeneous* propositions. Professor Ryle himself does not think that all dispositional statements require this kind of analysis. A possible compromise would be to suggest that few, if any, dispositional statements refer to an absolutely-uniform set of actions and reactions, but that all such sets have sufficient internal affinity to justify their being considered as logically-distinct classes. This reformulation of the phenomenalist theory seems to escape the main logical difficulties raised earlier, and in the rest of my paper I shall assume the adoption of this amendment.

I shall now pass on to the consideration of a different class of objections to the phenomenalist thesis. Roughly, these consist in pointing out the difficulties (familiar to all critics of the phenomenalist theory of perception) of offering a hypothetical analysis of statements which do not appear to have anything hypothetical about them. Statements which appear to ascribe some actual characteristic to some individual thing or person are "replaced" by statements about what would happen, or would have happened, *if* . . . A common criticism of this analysis is that no set of statements about possibilities, however elaborate, can be regarded as expressing fully the meaning of a categorical assertion.

Suppose, for example, one says of a musician who gives up the practice of his art during a certain period of his life, that his skill and artistry remained unexercised during this period. It seems here as though something categorical is being stated about the musician, and something else being denied about him. The assertion is that he retained his aptitudes and skills, that he "still had it in him" to give musical and efficient performances; the denial made is that he exercised these skills. Now according to the phenomenalist thesis, no characteristic whatever is ascribed to the musician by this statement. All that is being said is that the musician during this period *could have* played as he used to play in the old days, which means that he *would have*

played in this way if he had chosen to play at all, and that in fact he made no such choice. The phenomenalist analysis of the statement combines the assertion of a hypothetical proposition or set of propositions with the denial of the realisation of the conditions specified in the protases of these propositions. Thus the only difference between the statement that the musician retained his skill, and the contradictory statement that he did not retain it, is that certain hypothetical propositions are said to be true in the former case and false in the latter. The difference is a purely potential one which is doomed to remain such for ever, since the conditions necessary for the realisation of this possibility were not fulfilled during the only period when they might have been fulfilled. And in general, most of the differences which distinguish one person from another are fated to remain potential most of the time, if the phenomenalist thesis be accepted. When two persons are asleep, their potential differences are at their maximum, their *actual* differences are at their minimum. The difference between a dead man and a sleeping man, apart from differences in the observable processes going on in their bodies, is that the sleeping man *would* respond in various ways to various forms of stimulation, and the dead man wouldn't. This is the sort of distinction we are drawing when we say of the sleeping man that he *is* a good pianist or quick-tempered, and of the dead man that he *was* a good pianist or quick-tempered. The distinction between "is" and "was" is analysed as a distinction between "would" and "would not". In neither case are we saying anything unconditional about the man's present state and condition, when we assert or deny that he retains some dispositional quality.

Consider a comparable case. Suppose a piece of string and a pair of braces are lying alongside one another at the bottom of a drawer, one can draw attention to one of the differences between these articles by comparing the elasticity of the one with the inelasticity of the other. In making this comparison we are not, on the phenomenalist interpretation, saying anything about the actual properties or material composition of the two articles as they lie at the bottom of the drawer: all that we are saying is that they *would* react in typically-different ways if subjected to certain specifiable tests. But suppose they are not subjected to any such tests, what becomes of their difference then? No doubt it would still be true that *if* they were so tested, their reactions would be different: but surely it is also true that we mean to say something *non-potential*, *non-hypothetical*, about the string and the braces, when we compare the

elasticity of the one with the inelasticity of the other. We want to say that it is *in the nature of* the one, as it is not in the nature of the other, to react in those ways characteristic of elastic materials. The differences in their reactions to similar tests are referred to differences in their material composition ; and these latter differences, unlike the former, are conceived as relatively long-lived and stable. That is why they are called "dispositional" properties, because they predispose any object having these properties to react characteristically to certain specifiable tests. This pre-disposition remains as a property of the object, whether or not any such tests are being applied to it. It is probably true that we would not make any statements about the dispositional properties of objects and persons if we had not observed their different reactions to different situations and different tests, and noted certain consistencies in these reactions. But this does not imply that when we use dispositional language we are simply generalising these observed consistencies, or making condensed inductions about them. To suggest that this implication is true is, in my opinion, to confuse the meaning of statements about dispositions with the evidence which we use in making and confirming them.

Some of these difficulties in the phenomenalist position have been discussed by Professor Ryle in his book. He states as follows a general objection to the whole programme of talking about possibilities, capacities, tendencies, *et hoc genus omne* : "Potentialities, it is truistically said, are nothing actual. The world does not contain, over and above what exists and happens, some other things which are mere would-be things and could-be happenings. To say of a sleeping man that he can read French or of a piece of sugar that it is soluble in water, seems to be pretending at once to accord an attribute and to put that attribute into cold storage. But an attribute either does, or does not, characterise something. It cannot be merely on deposit account" (p. 119). Ryle thinks the edge of this objection can be turned by rejecting the myth that fact-reporting is the only function of significant indicative sentences. It is "pre-posterous" to assume that all sentences of this kind assert or deny that a certain object or set of objects possesses a certain attribute. Ryle compares statements about dispositions with statements of law. They resemble one another in their "open" or "variable" character, the analysis of which involves the notion of "any". Dispositional statements differ from statements of law in mentioning individuals. (If a label were required, they might be called "G I propositions", i.e. general propositions

about individuals.) The function of G I propositions is that they enable us to make predictions about, and give explanations of, the behaviour of individuals in different circumstances. They render unnecessary any speculations about unobservable states or processes, for the analysis of dispositional statements as G I propositions contains no reference to these.

This analysis seems to me to disregard the most important point about the use of dispositional language. The point surely is that the observable consistencies of behaviour, which individuals exhibit, appear to raise a problem which the language of dispositions is intended to meet, however inadequately it may perform this function. The problem is raised by the intermittent character, the "come and go-ness", of the episodes which are said to "satisfy" any given G I proposition. Long periods may intervene between one such episode and the next, during which the individual concerned may be doing and suffering lots of different things, apparently unrelated to one another and to what has gone before, and to what is coming afterwards; in some cases periods of deep unconsciousness intervene, during which there seems to be a dying of all activity except the most impersonal and biochemical: yet all these intervening events and conditions may make little apparent difference to the consistency with which the individual responds, in his characteristic manner, to a situation of a familiar kind. Dispositional hypotheses are an attempt to account for this stability and these recurrences by supposing that there exist relatively long-lived states of individual systems which, when acted on by relatively short-lived causes or stimuli, provoke a typical reaction from the system. Notice that "typical" does not imply "uniform". Different interferences with a system, when it is in a certain state, will generally evoke different reactions. Compliments and criticisms produce quite different effects on vain persons. Similarly, sunshine and rainfall have different effects on plants and people sensitive to climate. But this does not mean that terms like "vain" and "sensitive to climate" are heterogeneous. On the contrary, it is because they are used to refer to fairly-similar states and fairly-stable conditions that these characteristic differences of reaction to different types of interference can be relied on. The hypothesis that there are relatively invariable states or dispositions is intended to explain what may be termed the "consistent variability" or "systematic differences" of the individual's responses to varying circumstances. The predictability of these responses, and their subsumability under G I propositions, are regarded as derivative

from conservative and stabilising factors within each individual system, as it interacts with a less stable environment. Dispositional language is used not merely to collect together reactions of a certain kind, but to connect these together by means of a continuity-hypothesis which explains these reactions as the product of interaction between the stable properties of an enduring system and the variable occurrences which affect the system. In this way an explanation is offered both of the consistent variability of the individual's responses and their intermittent, spread out character. It is, of course, a very vague and rudimentary explanation-sketch, which merely sets a framework for further questions and enquiries, for more precise theories and more detailed observations.

It may be objected that these vague speculations have nothing to do with our ordinary use of dispositional language. Even if this were true, which I do not believe, the important problems would remain, as problematic as ever. If, as seems likely, phenomenalist analyses encourage us to avert our eyes from these problems, they can be criticised on these grounds alone.

The penalty for this refusal of the problem is the difficulty of giving any coherent account of the isolated incidents in an individual's life-history on which phenomenalist theories concentrate. Consider, for instance, the development of a disposition, such as skill at tennis. How is improvement possible, unless something is carried forward, as it were, from one game to the next? But if we confine our attention to those things which everyone can observe, our account of this development will consist in a series of descriptions of separate performances, and the connexion between these performances will be omitted. It is no answer to this to say that the connexion between these performances can be shown by subsuming this development of skill under some set of learning-laws, since these laws will either be generalised descriptions of the observable phenomena in particular learning-sequences, in which case the problem of the relationship between these phenomena will recur on a more general level: or else the gaps will be filled by hypothetical constructions, of the kind which phenomenologists consider unnecessary or illegitimate. The belief that such hypotheses are unnecessary may be encouraged by the comparison of dispositional statements with statements of law. One important distinction between G I propositions and statements of law is that the occurrences which "satisfy" the latter are assumed normally to be causally independent of one another, whereas this assumption of independence can never be made *a priori* about the

actions which satisfy a G I proposition. There is a problem therefore about the relationship between an individual's separate performances which does not arise in the case of the different occurrences which can be subsumed under the Law of Gravitation. The only way of avoiding hypotheses about dispositions is to ignore these problems, or to assume that those actions which are said to satisfy a G I proposition emerge out of the blue, and not out of a highly-organised system. The very notion of an individual appears to be unintelligible if we confine ourselves to a description of those activities which can be internally or externally observed, as a line of philosophers from Hume onwards has demonstrated. There is of course no easy solution of these problems available. But phenomenalist accounts of organisms do not begin to grapple with the fundamental difficulties. The thinness of their accounts is only partially concealed by their ventures in the realms of possibility. Such ventures cannot close the genuine gaps and discontinuities which partition off the related segments of an individual's history.

But, it may be asked, what alternative is there? Whatever the constructive defects of the phenomenalist attitude, surely these are preferable to the pseudo-explanations promoted by Faculty psychology? Is it not to the credit of phenomenalist logic that it has exposed the verbal pretensions of sham theories?

I agree that phenomenism exercises a cleansing influence in psychology, and has induced a healthy scepticism about theories which claim to reach beyond or "behind" the observable phenomena and to provide causal explanations which turn out on analysis to be mere redescriptions of the phenomena, with no predictive or explanatory power. But the cure for bad theories is better theories, not total abstention. No doubt one is much too easily satisfied if one accepts, as an explanation of a bird's nest-building, the statement that it does it "from instinct": but is it any better to be satisfied with the "explanation" that *all* birds of that species do it? Of the two sorts of answer, I slightly prefer the "instinct" sort, since this at least indicates where a better explanation is to be sought, namely in the individual bird or cat or man: whereas the other kind of answer diverts attention to the class or species, which is not an examinable entity. No one would start an experiment on all cats, and if he started it he would not finish it.

Can anything of a more constructive nature be said about dispositions? If they do not disappear between their appearances on stage, what sort of continuous existence can they be supposed to lead? Does John's irritability, when he is



not being irritated, continue to simmer beneath the surface as an unobservable turmoil, like volcanic activity which continues between eruptions ?<sup>1</sup> Or does the musician's skill, when it is not being exercised, consist in some kind of continuous internal practice, which keeps him in training for his next overt performance ? Are *these* the myths to which the abandonment of phenomenalism leads ?

No. The error here lies in regarding dispositions as internal replicas or continuations of the episodes which they are supposed to connect. To say that a person has a certain disposition is to say that he is predisposed to certain forms of response or reaction to certain conditions and circumstances ; and a predisposition to act is not itself an act but a kind of readiness to act. To think of dispositions as long-lived replicas of the short-lived responses which they partially determine, is like thinking of the well-tuned state of a piano as a non-stop inaudible sequence of tuneful notes. The state of readiness of a piano which has been prepared for a concert does not consist in any such sequence of events : no more does the readiness of the *pianist* for the concert, as he lies asleep in bed, consist in some corresponding sequence of events. I am not of course denying that pianos have to be tuned, and pianists have to practise : but the dispositional states of the piano and the pianist, which result from this tuning and practice, ought not to be conceived as an analogous sequence of activity going on *in camera*. It is important to distinguish between the pianist's *preparations* for the concert, the intermittent practice which he carries out, and his *state of preparedness* for the concert, which is the result of his preparatory activities. It may be misleading to speak of the "manifestation" or even "exercise" of a disposition, if these expressions are taken to imply that a disposition is an unobservable process or "frozen" reaction, of the same type as the observable manifestations of the disposition.

A relatively-simple, if untypical, example of a dispositional state is afforded by the "reflex" behaviour of organisms. The almost-unfailing regularity of these responses to definite forms of stimulation is attributable to the stable organisation of certain parts of the nervous system. This organisation predisposes the organism to react in definite ways to definite stimuli and does not of course disappear when the overt reaction ceases through the cessation of the adequate stimulus. Nor can this organisation be adequately described in terms of the overt

<sup>1</sup> Cf. I. Berlin, "Empirical Propositions and Hypothetical Statements", *Mind*, no. 235, p. 297.



behaviour which it makes possible : for instance, the organisation necessary for the knee-jerk reaction or the sneeze does not consist in some internal sequence of invisible knee-jerks or inaudible sneezes. It may be objected that it is unnecessary to know anything about the organisation of the nervous system in order to understand the meaning of statements about the permanent tendency of organisms to react in fixed ways to fixed stimuli. For the meaning of such statements can be unpacked in a series of hypothetical propositions, the protases of which specify the overt conditions necessary for the production of the overt responses specified in the apodoses. The explanation of a particular reaction consists in citing a general hypothetical proposition of the above kind, and in showing that the events preceding the reaction satisfy the conditions specified in the protasis. To this objection my answer is two-fold. First, I would agree that, in one sense of "understand", a man could be said to understand statements about the permanent tendencies of organisms to react in stereotyped ways to a narrow class of stimuli without having any detailed knowledge of physiology. But I would add that an expression like "permanent reaction-tendency" suggests the notion of a lasting state of unspecified nature, and that the physiological account is one possible way of developing this idea and making it more precise. A man might therefore be said to understand more clearly the implications of this expression, as used in the above context, when he became acquainted with the physiological theory. It is difficult in cases of this kind to distinguish sharply between the ordinary meanings of a term and its scientific use and associations. Secondly, concerning the phenomenalist "explanation" of a piece of reflex behaviour. It seems clear that as an explanation this is vastly inferior to the physiological explanation, and that if men like Sherrington had been satisfied with the former, an entire region of fruitful investigation and theorising would have been closed, and our understanding of reflex behaviour would have remained on a primitive level.

It is of course abundantly clear that no explanation approaching the adequacy of the physiological explanation of reflex behaviour can at present be provided by psychologists or physiologists for more complicated forms of human activity. For instance, no plausible theory, or sketch of a theory, is available that will delineate the nature of the state-differences which presumably distinguish the professional musician from the music-hater (assuming these to be different individuals). But unless such state-differences are assumed to exist, as well as the

observable difference in the reactions of the two individuals to musical occasions, it is difficult to see any prospect of an intelligible account of the consistency of these reactions. Without such an assumption, the individual episodes and performances scattered at intervals down a man's life will appear to have no more connexion with one another than our observations reveal, and the fact that many of these episodes can be subsumed under G I propositions must be accepted with piety as an ultimate mystery. The assumption that there *are* such state-differences may seem to incur Russell's scathing criticism, that the method of postulating what we want has the same advantages as those of theft over honest toil. But this criticism can be avoided by ensuring that hypotheses do an honest job and perform a useful service as guides and stimulants to inquiry, and not as its termini. How they are to be made to do this job effectively in the present case, I do not know. But unless such hypotheses can be constructed, little progress will be made in the understanding of human conduct and dispositions.

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## V.—PRIVILEGED UTTERANCES

By D. MITCHELL

SINCE at any rate the seventeenth century it has been commonly assumed that our knowledge of the world is acquired either by observing, in an ordinary unmetaphorical sense of 'observe', or by 'observing' in some metaphorical non-sensory sense, by sensation on the one hand or by intuition, reflection or introspection on the other. Yet, although both empiricists and their opponents have, as a rule, taken it for granted that these are the only sources of such knowledge, their account can be shown to be incomplete. In one sense, both sides in the argument have come very near to subscribing to Professor Ayer's statement, "there is no field of experience which cannot in principle be brought under some form of scientific law and no type of speculative knowledge about the world which is, in principle, beyond the power of science to give". To say that a field of experience can be brought under some form of scientific law is to imply that I can speak meaningfully only about what I can observe, scrutinize, weigh, measure, put under a microscope. But, while the pure empiricist has admitted the possibility only of sensory observation and scrutiny, his opponent has urged that there are some things that can be observed, some facts that can be established, particularly certain facts about ourselves, by non-sensuous introspection or intuition. So, as I can talk of tables and chairs because I can see them, I can talk meaningfully of hopes and fears because, in Locke's terminology, I have not only ideas of sensation but also ideas of reflection, whereby I perceive the operations of my own mind within me. But what both sides share in common is the assumption that for anything to be a possible subject of discourse it must be susceptible of *some* sort of inspection and observation, whether sensuous or non-sensuous; and to this extent both can be usefully described as verificationists. Perhaps it is this joint acceptance of the relation of meaningfulness to observation and verification that has led many philosophers to regard the 'private access' and the 'public access' views (to borrow Professor Ryle's terms) as exhaustive and not so much to reject the view here discussed as not to consider it at all.

Yet, that both of these views are inadequate is apparent from a critical consideration of Locke's 'ideas of reflection'. On the

Lockian view feeling unwell, feeling happy, for example, are events in my life history of which I become aware not by sight or touch but by reflection, by an inner 'looking' with a non-sensuous 'eye'. On such a view, feeling unwell is one thing, 'observing' oneself feeling unwell is another. But can an event describable as feeling unwell be distinguished from the awareness that I am feeling unwell? Would it be possible to see without being aware that I was seeing? Surely not. If seeing were something that could be distinguished from consciousness, from 'awareness-of-seeing', it would not be seeing at all. By 'seeing', 'feeling unwell', 'intending to do  $x$ ', and so on, we do not intend processes, states or activities which could occur unaware. To feel pain is to be aware of pain, seeing and feeling are not states of which I am aware, they are not possible *objects* of awareness, they are two of the forms which awareness takes. I could not feel depressed unawares, and to declare that I am depressed is to declare a state of awareness which is its own guarantee and needs no further awareness to support it.

It is not difficult to see that declarations of such conscious states and activities differ widely from statements of observation, whether of 'outer sense' or (if there be such a faculty) of 'inner sense'. When I say "I feel unwell" I do not *discover* or *conclude* or *take it for granted* that I am feeling unwell. Yet, if feeling unwell were the object of observation of any sort, one or other of these verbs would properly describe what occurred. When I honestly declare that I am feeling unwell, when I am not pretending or lying, I do not draw a conclusion from evidence, not even from *conclusive* evidence. It is not a matter of evidence or grounds at all. Similarly, it would be nonsensical for me to say that I believe that I am feeling unwell; and, although common usage sanctions the use of 'know' in such sentences, there is an awkwardness in "I know that I am feeling unwell", which has the same origin. I can believe or know that I have appendicitis, for it is possible for me to have appendicitis unawares, but it is not possible to feel unwell unawares. In other words it is impossible to feel unwell without feeling unwell. Having appendicitis is a fact that is, so to speak, waiting to be discovered, being unwell is not. To say, "I believe that I feel unwell" or "I know that I feel unwell" implies that I could feel unwell without knowing or believing that I feel unwell, which is absurd. It is true that on occasions I may say, "I suddenly discovered that I had a pain", but I do not mean by that that I discover now that I had a pain all along but either that I suddenly feel a pain or that, although I have

been (consciously) in pain all along but with most of my attention taken up with something else, my pain becomes the centre of my attention. Thus, if I tell you that I am in pain it is not sensible for you to say, "how do you know you are? What evidence have you for saying that? I wonder if you may not be mistaken". Such comments are absurd not because the evidence is so clear that no reasonable man could misinterpret it. They are not absurd as it would be absurd in ordinary life, if I asked an Oxford undergraduate for evidence for the statement that the Vice-Chancellor is not so tall as the Radcliffe Camera. They are absurd, of course, because there is no question of evidence at all. My feeling pain is not an object of *my* awareness, *my* scrutiny, *my* observation any more than it is an object of *yours*.

The fact that there is no connexion between declarations of conscious states and evidence makes it apparent why they are not to be classified as 'basic propositions'. For the property of the alleged non-analytic propositions which have been called 'basic' is that they can be conclusively verified; in Ayer's words, "they refer solely to the content of a single experience, and what may be said to verify them conclusively is the occurrence of the experience to which they uniquely refer". In fact, there is an awkwardness even in calling these declarations 'statements'. If I say that I am feeling depressed I may be making an honest declaration or telling a lie but to call my utterance a statement is to suggest that it might be true or false (in the sense of 'mistaken') in the same way that 'The cat is on the mat' might be true or false. In spite of the fact that I may express myself clumsily and, hence, mistakenly ("Don't you really mean 'worried', not 'depressed' ?"), and that, it may be, all human utterances are susceptible of *some* error, to classify the declarations of conscious states as, in certain respects, infallible is useful in that it underlines the distinction between non-analytic informative utterances which are not, from those which are, verifiable and based on evidence.<sup>1</sup> From all these considerations it seems to follow (1) that, since it is senseless to ask for evidence for declarations such as 'I am in pain', and since, if we hesitate to accept them, it is because we suspect an intention to deceive and not error, the speaker is privileged and alone can inform us of his conscious states; (2) that his privilege does not derive from a 'privileged access' which he has to face facts beyond the range of others' 'vision'. For although the

<sup>1</sup> It is in this weakened sense that I shall speak in this paper of utterances as being 'infallible'.

possession of an inner sense would explain why I, but not you, should be able to say how I feel, it would not explain the fact that it is senseless to ask for evidence for such declarations, for if I had such a faculty it would always be sensible in philosophical discussion to question whether I had in fact misinterpreted the evidence, drawn a false conclusion about the nature of what I 'saw'.

Professor Ryle in *The Concept of Mind* argues strongly against both the para-optical, inner-sense view implicit, for example, in Locke's doctrine of Reflection and also the view that there are mental events which are different from physical events. To consider briefly his conclusions may serve to clarify the view that has been put forward here. In discussing the 'operations of the mind' which are the alleged objects of reflection, he says ; "The imputed episodes seemed to be impenetrably 'internal' because they were genuinely unwitnessable. But they were genuinely unwitnessable, because they were mythical" (p. 318). He argues, on the contrary, that "the sorts of things that I can find out about myself are the same as the sorts of things that I can find out about other people, and the methods of finding them out are much the same" (p. 155). I have tried to show that I agree with Ryle that we do not learn about our conscious states and activities by means of a faculty of inner sense ; but here the agreement ends. For Ryle, since we have no faculty of inner sense and since we can perceive the alleged operations of the mind by no other faculty, we mean by 'operations of the mind' no more than modes of observable behaviour ; mental vocabulary is simply an alternative to a physical vocabulary that applies to the same range of objects. His conclusion seems to rest on the assumption that if there were 'mental events' (in the sense rejected by him) they would have to be witnessable, since by observing things we find out about them and discover their nature. I have tried to show that in declaring our conscious states and activities we are not declaring the results of observation at all, whether outer or inner. The physician uses this method or that for finding out if his patient is genuinely in pain, but the patient does not find out if he is in pain and *a fortiori* does not use the same methods of finding out if he is in pain as the physician.

The fact that conscious states are not witnessable is not an adequate reason for describing them as mythical. But a materialist might still think that strong enough reasons have not been given for rejecting the view that 'conscious states' is an alternative general description of certain types of observable

behaviour. But the case that he has to maintain is a difficult and paradoxical one. A man can declare if he is feeling angry or depressed and the extent to which we accept his declarations is determined by the belief we hold in his sincerity. But the physiologist does not *declare* how the brain functions and if we question his account it is because we doubt not his sincerity but the accuracy and completeness of his observations. We do not need to have heard of the retina to understand what sight is: what it is, further knowledge of the structure of the eye will not teach us. In a world in which all but a handful of men were born blind, although the blind might know the correct uses of the word 'see', both in its literal and transferred senses, although they would be able to give the physiologist's account of sight and devise tests for deciding whether, when the sighted alleged they saw, they were telling the truth or lying, they would not know what it is to see, what the sighted meant when they said that they saw this or that. A necessary condition for understanding what seeing is, or feeling depressed, or hoping, is for oneself to have seen, felt depressed or hoped. To accept the common identification of thought with observable changes in the brain and nervous system is to be guilty of a far greater absurdity than that of Locke's "studious blind man" who thought that the colour scarlet was like the sound of a trumpet. For the blind man realised that to see was to experience and was right in looking for his parallel to sight in another form of experience, namely hearing. To identify thought with observable change is to confuse experience with its objects, sight with what can be seen.

Seeing, hearing, thinking are forms of experience, yours or mine (but never nobody's); observable changes in the cortex are not. The latter may be, the former can never be studied scientifically. Physiologists at different times have placed the mind in the liver, the spleen, the pituitary gland, the pineal gland, the brain or part of the brain but the meaning of the word 'think' does not change with physiological theory. No new discoveries about the structure and function of the eye can throw light on the meaning of 'see'. Since, then, when I declare that I am in pain I do not declare that such and such an observable change occurs, to state or deny that that change occurs is not to state or deny that I am in pain. My being in pain is an event in my life-history but not the same event as any physical condition or change that occurs when I am in pain.

To assert that there is no field of experience which cannot be studied scientifically is not to abandon the philosophy of mind

to the physiologist, for the topics with which the philosophy of mind is concerned, thinking, deciding, seeing, feeling, hoping, believing, do not constitute a field of experience. To describe thought as a field of experience is to commit one of two fallacies, either (1) that of identifying changes in the nervous system (a genuine field of experience, where observation and measurement are possible) with thinking, or (2) that of Locke, that I know about thinking by 'observing' myself thinking, whereby 'thought' is transformed into a parapsychical object studied by a parascience of introspection.

What consciousness or awareness is, we understand by ourselves being in conscious states and engaging in conscious activities, by feeling pleased or hurt, by hoping, intending and deciding. Just as the awareness that characterises these states and activities is not elucidated by the *identification* of them with physical processes, so it is not elucidated by drawing *analogies* between them and physical processes. The absurdity of translating 'I think' as 'there is a thought now' is obvious. We should only understand 'there is a thought now' if we already knew what it was to be able to declare 'I think'. The philosopher's motive for inventing such reformulations is to be traced to the assumption that for a declaration like 'I think' to be philosophically respectable it must be shown to be the same sort of utterance as 'An apple fell from the tree', that is to say, a statement of fact which can be verified by observation. Our philosophical traditions have encouraged us to feel a perplexity about autobiographical utterances that disappears as soon as we recognise that we have been adopting the wrong model of intelligibility. Aristotle's account of motivation is refreshing not least because he did not attempt to make it intelligible by misleading analogies with causal physical change. Even after three hundred years of verificationist philosophising it takes a good deal of tutorial therapeutics to persuade the ordinary man that because we do not seem to be saying the same sort of thing by "For such and such reasons I decided to do *x*" as by "His skull was cracked by a blunt instrument", that the former needs to be reinterpreted to be fully intelligible.

It would be absurd to claim that all autobiographical utterances are privileged in the way that "I am feeling unwell" is privileged. Ryle assumed that "I am good at long division", is an utterance of the type that his opponents would wish to claim to be privileged, and his argument that it may be as significantly contradicted or asserted by others as by the original speaker, is intended to show that the distinction between privileged



and non-privileged autobiographical utterances is illusory. Of course it is true that a conscientious schoolmaster is a better informant on a boy's capacity at arithmetic than the boy himself. But one would not wish to claim that statements about one's capacities are privileged though one would make such a claim of "I look forward to doing long division". Similarly, declarations about my past experiences are of course fallible and unprivileged by contrast with those we make of our present experiences. Briefly, the utterances which are privileged are those to which the addition of such adverbs as 'knowingly', 'awares', 'unwittingly', 'unawares' is tautologous or contradictory.

Yet the class of conscious states and activities, our avowals of which are privileged, is a large one, and, particularly in ethics, a failure to distinguish such conscious states from observable changes has been a source of confusion. What this confusion is may be made clear by a consideration of the different ways in which we use certain active verbs which take personal subjects, which I shall call 'doing' verbs. These may be classified under three heads, thus :

Class I—Such verbs as fall, digest, slip, die.

Class II—Such verbs as jump, hurl, assassinate, pursue, flatter.

Class III—Such verbs as kill, knock, hit, move, jolt.

In cases of 'Class I—doing' we look for causes not reasons. We ask 'what caused his death?' not 'what was his reason (or motive) for dying?' In cases of 'Class II—doing' we look on the other hand for reasons not causes. In cases of 'Class III—doing' the verb by itself is ambiguous. Thus to the informative sentence, "A drove his car into B and killed him", the appropriate response might be either to investigate the cause of an accident or to attempt to discover the motives of a murderer. In order to discover which is the appropriate response I must discover whether 'kill' is being used as Class I verbs are used or as Class II verbs are used. 'Doing-verbs', then, are used in one of two ways which I shall call Sense A (for Class I verbs) and Sense B (for Class II verbs), Class III verbs being used in either sense. 'Class I—doing' is never, 'Class II—doing' is always 'on purpose'. But to say this is not to add anything to what has been said by these verbs; for the function of the words 'on purpose' and 'accidentally' is simply to indicate the sense (A or B) in which a verb is to be understood and therefore, where no ambiguity is possible, as with 'digest' (Class I) or 'pursue' (Class II), these expressions are not used. At first

sight the objection to conjoining them with Class I or II verbs is simply that they would be redundant, but in fact it would also be misleading in that it would suggest that an unambiguous word was in fact, ambiguous. To say "A pursued B on purpose" suggests that A might have pursued him accidentally. "A accidentally died" suggests that it might have been the case that he died on purpose. It is possible to conceive a language in which Class III verbs were eliminated altogether and one result of such a 'tidying-up' process would be that we should be less inclined to think that 'doing verbs' refer to neutral events which are sometimes intended and sometimes accidental. We should see more easily that when in the unreformed language we said 'Did he do it on purpose or did he do it accidentally?' 'do' in the first question was a misleading but not accidental homonym of 'do' in the second and that the meanings of the two were quite different.

It is not difficult to show that we sometimes use the verb 'do' itself without intending to convey the information that any physical change is taking place. Imagine that Smith is standing at the window of Brown's room and gazing into the street below with a puzzled look on his face.

*Brown.* "What are you looking at?"

*Smith.* "A man in the street down there."

*Brown.* "What is he doing?"

*Smith.* "He's waving his arms about in the most extraordinary way."

Brown goes to the window and is equally puzzled and says, "Whatever can he be doing?"

It is clear that a legitimate and sensible answer to the first question would consist (partly or wholly) of a description of the observable physical movements of the man in the street. But to include such a description in an answer to the second question would be to betray a misunderstanding of the meaning of the word 'do' in the context. Its function here is to indicate an event in a man's life history but not a physically witnessable event. His behaviour is in principle fully observable, what he is doing (in Sense B) is not. It is of course true that we are not usually puzzled to explain a man's observed behaviour and that, when we are puzzled, a closer look usually clears up our doubts, but this fact should not allow us to confuse statements about behaviour and statements about what a man does in Sense B. The former need not be, the latter must always be essentially inferential. In the same way, to declare what I do in Sense B,

is to make an infallible avowal of the same type as "I am feeling depressed", "I want to go home", "I intend to go home". So that when I declare what I do in Sense B there is no danger of making a mistake through misinterpretation of evidence since there is no evidence to be misinterpreted.

However, there is a difficulty about this account. If Brown were to ask the man in the street what he was doing, few, if any, of the specific answers that the man might give would appear to be classifiable as 'pure' avowals. Although Brown is not enquiring about the man's behaviour, a possible answer such as "I'm sending a message in semaphore" implies that behaviour of a certain type occurred. In principle, there is a place for error. In this respect, then, specific declarations of what I do seem not to belong to the same class of privileged utterances as 'I feel depressed', 'I want to go home'. Yet this difficulty is surmountable and I hope to show that in accounts of human conduct there are isolable, distinguishable events that occur which are quite distinct from physical behaviour. Such events are those which we declare in some but not all sentences which include the phrase 'try to' or 'trying to'. When A runs over B with his car and kills him, the question that the detective asks himself is, "Was it an accident or was A trying to kill B?" Now the point to be emphasised is that this use of 'trying' is not the only or the commonest use of the word. To say that A tried to do *x* usually means that A took the means in his power to achieve *x* as a result. Thus, if I run a race, one may say, in the common use of 'try', that I am trying to win. To say, in the same sense, that I am trying to run would be nonsense, unless I were, for example, crippled, paralysed or heavily burdened. If I, an unparalysed man, raise my unencumbered arm to remove my hat I do not, in the common sense of 'try', try (*i.e.* take means) to raise it. But the sense of 'try' to which I wish to draw attention is different from this, for it is a sense in which it *could*, in certain circumstances, be said that I, an unparalysed man, try to raise my arm. It is, as I have said, in this and not the common sense of the word, that the detective asks himself if A was trying to kill B. For the detective is concerned to decide not if A was taking the means in his power to kill B but whether he is investigating purposive activity or an unpurposed causally explicable series of events. His question, in other words, is not "What was A trying to do?" (in the common sense of 'try'), but "Was it a case of 'trying-to-do' anything at all?" (*i.e.* purposive activity). In parenthesis it may be noted that we use other verbs than 'try' with the

same force, in particular the verb 'mean'. Thus if I am not sure if the movement of your arm is intentional or not I may say 'Were you trying to raise your arm?' or, (perhaps more commonly), 'Were you meaning to raise your arm?'

In the past, consideration of certain problems of moral obligation has led some philosophers to argue that, in a full description of action, place must be found for an alleged mental act (or activity) that was called 'setting oneself' (to do  $x$ ), distinguishable and in a sense intermediary between decision (a mental act) and performance (physical behaviour). At first reading it may appear that the alleged events in men's life-history which I have called 'doing' (in Sense B) or 'trying to do' consist, in fact, of the old-fashioned 'setting oneself' under other names. But this is not the case. What I am attempting to show is not that between decision and performance some third act or process intervenes called 'trying to do', but that the meaning of 'doing' has been consistently misunderstood, that just as decisions are 'mental' so actions, also, are 'mental', that there is no need to posit a link between the mental and the physical, since to assert that one passes from decision to action is not to assert that one passes from a mental activity to a physical activity. Professor Ryle has rightly objected to the common version of the 'two world theory' according to which mental events (decisions) cause changes in the physical world, the movements of my arm, on such a view, being caused by my decision to raise it. The relation between decision and physical change, between the world of thought and the world of physics is completely mysterious. The headless ghost that walks through stone walls is not so disturbing as the ghost that barks its shins on a door-scraper. Yet, of course, when I go through the process of thought called 'deciding to open the door' and then put that decision into effect, my legs and arms move in harmony with my decision and the door turns on its hinges at the pressure of my hand. It is not surprising, therefore, that where so absolute a harmony exists, philosophers have sometimes alleged that my decision *caused* the opening of the door. But the relation between deciding and doing is not mysterious, because, 'doing on purpose' is not a physical change at all. The 'doing on purpose', the actions that we praise or blame, our declarations of which are infallible avowals, not the fallible reports of observation or discovery, are what I have tried to show we refer to by such phrases as 'meaning to do', 'trying to do', 'doing' (in Sense B). When the detective, investigating B's death under the wheels of A's car, asks himself 'Was

A trying to kill B?' he is not trying to discover if A had an intention or reached a decision which caused the physical movements (the depression of the accelerator, etc.), which brought about the death of B. Nor again is he *merely* concerned with deciding whether A reached a decision to kill B. He is concerned to discover if A *put such a decision into effect*. If he concludes that A should be charged with wilful murder he will have decided that A put a decision into effect, that is to say, that A was, after all, in the special sense of 'trying to' that has been discussed, trying to kill B. For wilful murder is not 'intending to do' but 'doing'.

Thus the paradoxical account of human conduct according to which mental events cause physical changes, to which Ryle and others reasonably object, is not entailed by the contention that to have an intention is a 'mental occurrence', since the actions which are the putting into effect of intentions are not physical events but equally mental. A full discussion of the proper use and application of ethical terms to human conduct involves no reference to the problem of relations of mind and body, since both actions and intentions (and motives, too) belong, in Ryle's language, to the vocabulary of the same world, that is to say the 'mental world'. How it is that when I decide to adopt a course of action and adopt it (in the sense of 'adopt' which does not denote a physical operation), my bodily movements 'correspond' with my intentions remains a puzzle. Perhaps the biggest puzzle is "How, in any case, should we set about trying to solve it?" But to attempt to dispel the puzzle by denying the 'mental' character of such experiences as deciding, intending and so on, and by interpreting the language of decision, intention (and, for that matter, thinking and feeling), behaviouristically, it has been part of the purpose of this article to show to be an inadmissible procedure.

One further objection remains which must be briefly considered. It may be argued against the view that our declaration of intentions and purposive actions are privileged and cannot be mistaken in the way that empirical statements can be mistaken, that we sometimes act from motives (or reasons) of which we are not conscious, and that it is even possible for a man to do something on purpose which, at the time, he would describe as an accident. In such situations others (and, in particular, experienced psychiatrists) can better judge our intentions than ourselves. It might seem reasonable to conclude, then, that we are not in any specially privileged position to declare our intentions, at least in some cases, and, if not

in some, then perhaps not in any. On such a view the distinction which has been drawn between empirical statements and privileged autobiographical declarations breaks down. But to adopt it would be a mistake. For if we consider, for example, Freud's treatment of the 'psychology of errors' we can see that the line of argument, which appears to give ground for the denial of the distinction between empirical statements and privileged autobiographical utterances, itself depends on that distinction for its cogency. For, as Freud says, the only conclusive evidence in support of the hypothesis that a great proportion of slips of the tongue or the pen, mislayings, forgettings, etc., are subconsciously intentional is that we admit that they are intentional when such a suggestion is put to us. "You shall grant me", he says, "that the meaning of an error *admits of no doubt* when the subject of the analysis *acknowledges* it himself. I, in turn, will admit that a direct proof for the suspected meaning cannot be obtained if the subject refuses us the information" (*Introductory Lectures to Psychoanalysis*: English edn., p. 39, *my italics*). In cases where the patient himself is unable or unwilling to acknowledge that the slip in question was intentional, Freud allows that no conclusive proof can be produced but that we must argue by analogy. But what justifies the analyst in drawing the analogy is the fact that in other similar cases the patient himself acknowledges the intention attributed to him by the analyst. That a man's own admissions are the final authority and are of a different weight and status than the analyst's hypothesis is accepted and is, in fact, crucial. It seems, further, that one of the chief aims of the technique of the psychoanalyst is to bring the patient to the point where he can acknowledge his motives and thereby confirm or correct the original diagnosis. It is not insignificant that the language in which the psychologist speaks of the subconscious and conscious is the language of 'reasons' (motive, intention, purpose, conscious, subconscious, unconscious) not the language of 'causes'. To assert that on occasions men may properly be said to acknowledge their subconscious intentions and the purposiveness of their 'errors' is to recognise implicitly that a man's declaration of his own motives are privileged.

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## VI.—FREE WILL AND DECISION

BY JUSTUS HARTNACK

SELDOM, if ever, does an analysis succeed in clarifying a problem to such a degree that general assent results. On the contrary, often other analyses are set forth which claim an opposite view. This, at least, has been the case with the problem of the freedom of the will. Such freedom has by some been vigorously affirmed, by others vigorously denied, and finally has by some been declared a pseudo-problem. I intend to argue in this paper that none of these standpoints are wholly incorrect, that each of them is asserting something true; that, in other words, the philosopher who asserts freedom is, more or less successfully, giving expression to something true while the philosopher who is denying this freedom is denying what some may have falsely asserted, and that finally some metaphysicians have tried to assert something which could be properly called meaningless.

What do we mean by saying that man has a free will? A generally accepted answer is that the will is free if at a given moment a certain person could have chosen to act otherwise than he in fact did. This assertion is what we now shall have to clarify. If I say that I in a given situation could have chosen to act otherwise than I in fact did, what do I mean? Suppose somebody asks me why I did not go to a meeting, what he is asking for is a reason. He expects me to say something like: "Because I didn't feel well" or "Because I was too busy correcting papers". That is, whenever we speak about choosing one action or doing instead of another we are maintaining that there is a reason for our choice. What would it mean to speak about a choice for which there was no reason? To be sure, there is a big class of actions which I normally perform without any reason. I breathe, I sneeze, scratch myself behind the ear without knowing it, I blink my eyes, etc. Normally I do not choose or decide to blink my eyes and I do not choose or decide to breathe and it would be absurd to ask why I do so. Of course, if a child asks me, "Why do we breathe?" I may answer "Because if we did not we would be suffocated". That this kind of reason, however, is quite different from the kind of reason relevant to choices or decisions is obvious. Compare, for instance, the two questions "Why do you breathe?" and "Why do you take sleeping pills?" In both cases do I give a



reason. The answer to the first question is "In order not to suffocate". To the second question it is "In order to sleep". But the important difference is that I take the pills because I know or have good reasons to believe they will help me in going to sleep and that it would be absurd to say that I breathe because I know it is necessary for not being suffocated. We would breathe even if nobody had any knowledge of its necessity but nobody would take sleeping pills unless at least one person had the adequate knowledge.

What we have established so far is that it is relevant to speak about freedom only in cases where we can account for our actions by referring to reasons, *i.e.* the reasons for our actions.

Now, granted that there is always a reason for actions in cases where we meaningfully speak about freedom, it is then possible that someone may ask the question whether we in a given situation decide to act from the reason given or whether there is no such decision. If we say that there is such a decision we face the question whether we could have decided otherwise than we in fact did or whether we could not. I shall try to show that this question is a pseudo-question. If I answer by saying that we could have decided otherwise, then what I am saying is that I could have decided otherwise if I had had a reason to do so (because, as we have seen, without referring to a reason it is irrelevant to the question about freedom) and that leads me into the following absurdity: If I have a reason to act in a certain way I do not act so if I have a reason not to do so! But I cannot say either that I could not have decided to act contrary to my reason for acting in a certain way; because that leads me to say that if I have a reason for acting in a certain way I have no reason not to act from the reason given, which is absurd, because I can then ask whether I have reason to accept a reason for a reason which in turn is a reason for another reason and so on *ad infinitum*. The conclusion is that it is nonsensical to say that we decide to act from the reasons for our actions; all we can say is that we *do act* from such reasons.

But this is unsatisfactory; because it seems to eliminate decisions altogether. We cannot say that we decide to act from our reasons and we cannot say either that we have the reasons we have because we have decided—that would be just as absurd as to say that we have decided that headaches are unpleasant. It seems, in other words, as if we never decided. To say that is unsatisfactory because we do, in fact, decide. We decide many things every day, each one of us, and in such



a way that we are free in our decisions. My getting up in the morning is usually a decision. Should I wait two more minutes or should it be now? Should I walk or should I take the bus? Should I go to the theatre or should I stay at home reading? To say that we in such situations do not decide is simply false. Our philosophic duty is neither to deny that which is true nor is it to affirm the truth. It is to understand what it is we are talking about when we, for instance, say that we decide. However an analysis of decision and freedom may turn out, it cannot change the fact that there are situations which it is accepted usage to describe in terms of decision and freedom.

In order to make this clear let us compare two different situations: (1) I am walking to the station. I look at my watch and discover that I cannot get the train without running and as it is important to me to catch the train I decide to run. (2) I am trying to see how long a time I can hold my breath. After a certain time I feel tempted to give up but resist the temptation. But finally I have to give in and I find myself breathing.

Let us now compare the two questions. (1) "Was it possible for you in this particular situation not to have decided to run?" (2) "Was it possible for you in this particular situation not to have decided to breathe?" The obvious answer to the first question is "Of course it was, but then I wouldn't have caught the train and that was what I wanted". The answer to the second question is likely to be "I didn't decide to breathe at all. On the contrary, I had decided not to breathe but it was impossible for me to go on." Now, in so far as we agree that these two answers are acceptable answers, they affirm that it is correct to talk about freedom and decision. But we are not interested only in knowing what we can say; we are also interested in knowing what it is we are referring to when we are talking. Why is it that I say that I decided to run while I did not decide to breathe? In order to answer this question it is important to be aware of the similarities as well as the differences between the two situations. One similarity is this. I am in both situations able to point to certain circumstances as the reason. If somebody asks me why I started to run and why I started to breathe I am in a position to answer. My answer to the first question is "Because there were only two minutes until the train left" and my answer to the second question is "Because the oxygen content of my blood was so low that the stimulation to breathe overcame my efforts not to breathe". Thus, both in cases where we speak about decisions as well as in cases where there are no decisions, there are certain

events or occurrences which occasion or cause the actions. Let us now examine the important differences. While I can say that I tried as hard as I could to hold my breath but finally my resistance broke down, it would be clearly absurd to say that I tried as hard as I could to go on walking but finally my resistance broke down. This is absurd because, in that sense of 'impossible' in which it was impossible to hold my breath, it was quite clearly not impossible to continue to walk. I in fact decided to run. What is it that lets us speak about a decision in the one case while we denied it in the other case? To bring this out more clearly let me compare my decision to run to the station with a situation where I start to run but where I could not speak about making a decision. I may start to run, for instance, without thinking of it at all, in which case it would be plainly false to say that I decided to run. I may, so to speak, discover that I have started to run.

The difference between these two situations is this. If I had not arrived at the conclusion that it was necessary to run in order to catch the train, I should not in this particular situation have started to run. However, even if I know that I shall not catch the train, I may nevertheless not decide to run. I shall only do so if I *want* to catch the train. I may, for some reasons, be indifferent whether I catch the train now or the train twenty minutes later and if that is the case I shall not start to run even if I conclude that I must run in order to catch the train. But if I do want to catch the train and if I do know that the only way to catch it is by running then I do run. If I start running without thinking of it, on the other side, then clearly there is not an explicitly formulated purpose, goal or aim such as in the other case where I wanted to catch the train. Nothing of the sort can be formulated in this case; my running is not related to any such purpose. Therefore, whatever information I may have got, whatever conclusions I may have arrived at, that could never have occasioned a decision to run simply because we do not speak about decisions except when a conclusion arrived at occasions or causes an action because it is relevant to an explicitly formulated purpose.

This seems, then, to be the essence of all decisions, namely, that we arrive at a conclusion about what to do in order to accomplish a certain purpose.

It may be objected that this definition of what a decision is is an oversimplification. It is indeed an oversimplification; but it is not a falsification; it is in principle true. When decisions are difficult to make it is either because we do not

possess sufficient information to arrive at a conclusion or because we have conflicting purposes, wants, needs, wishes, propensities, or desires. The doctor's difficulty in deciding whether he should operate or not, or the businessman's difficulties in deciding whether he should sell or not, are due to lack of sufficient information. If the doctor finally decides to operate, it is because he has arrived at the conclusion that there is a higher probability of the patient's recovery by operating than by not operating. If the businessman decides to sell, it is because he has arrived at the conclusion that selling will secure him a higher profit or lesser loss than not selling.

On the other hand, whether I go to the concert or stay at home working on an article may be difficult to decide, not from lack of information, but because there are two conflicting wishes. I do not want to miss the concert but I wish to have my article done before tomorrow morning and I do not want to go to bed too late. If I decide to stay at home, it means that my desire to have the paper finished before tomorrow morning is stronger than my desire to listen to the concert (because what we mean by saying that one desire is stronger than another is precisely that we are ready to sacrifice the satisfaction of the latter for the satisfaction of the former). My decision, then, to stay at home is determined by the desire to have the paper finished before tomorrow morning which cannot be accomplished if I go to the concert.

Now it is often maintained that the reasons I give for my actions are not always the real reasons. Not that I do not believe myself to be honest, but that a further psychological analysis reveals that I was mistaken in my belief about the reasons. What I am supposed to be mistaken about is not my beliefs about the factual situation but my beliefs about my wishes and desires. This may very well be the case but it has no bearing on our definition of decision. If I have no knowledge of the wish or desire according to which I act, I do not—*ex hypothesi*—engage myself in any inquiry about the satisfaction of that desire or how to overcome obstacles; if conflicts arise I may get neurotic but I do not undertake any examination of how—if possible—to compromise. In short, I do not arrive at any conclusion, not because the situation is too difficult but because there is no inquiry. It is incorrect, therefore, in such cases to speak about decisions.

It can now, by way of conclusion, be seen what is meant by saying that the will is free. To assert the freedom of the will is to assert that there are situations where a person decides what to

do. To say that he could decide otherwise than he in fact did is to say that if his desires and wishes had been otherwise his conclusions about what to do would have been different. These conditions were satisfied when I started to run in order to catch the train but were not satisfied when I started to breathe. If I had had different desires or wishes in the former case I would have decided otherwise, but my desires or wishes would have made no difference in the latter case. *This, then, is what is referred to when it is maintained that the will is free.* It is consequently false to deny freedom, because that would be to erase the difference between cases where I act without deciding to do so and cases when I do so decide. If it should be maintained that to be free means to go against all my wishes it would be absurd. Because if the act of going against all my wishes is an act of freedom it must be from a wish or a desire to do so ; consequently it could not be an act going against all my wishes. It would be quite absurd if somebody said : " I decided to stay at home because I did not wish to stay at home." Likewise, if it should be maintained that the phrase " If I had wished or preferred to do so " in the sentence " I could have decided otherwise if I had wished or preferred to do so " implies that I could have decided to go against my wishes—the very wishes I intended to satisfy by the act I decided to do—it would be either absurd or mean nothing but that I might have had wishes that were stronger than the wishes I in fact intended to satisfy by the action I decided to do. This seems to be just as interesting as the statement that it might have been the case that I should have preferred coffee to tea instead of, as it is now, tea to coffee.

A few words should be said about moral responsibility and repentance.

We hold a man responsible for the acts he has decided to do or decided not to do. But we also hold him responsible for some acts which he performs and which he has not decided to perform. Swearing, for instance, is usually not a result of a decision ; I swear without thinking of it. Whether we call such actions voluntary or involuntary is not a matter of great importance ; what is important is this. Each time I swear it had been possible not to swear if I had decided not to do it in this particular sentence (such a decision should be clearly distinguished from the decision I make to stop swearing the rest of my life. I may make such a decision but it is not at all certain that the decision will have the intended effect). This is the way they differ from such clearly involuntary actions as for instance sneezing. I may try hard not to sneeze but all my efforts may be in vain. It

makes sense, therefore, to say that I could not help that I sneezed. But it would not make sense to say that I tried hard not to swear in this sentence but I could not help that I did. Likewise we do not say that I tried hard not to spit but that, nevertheless, it was impossible not to spit or that I made great efforts not to interrupt his speech but all my efforts were in vain. The point is that we are held responsible for acts we did not decide to do if by decision we could have acted otherwise.

We are finally held responsible for things we should have done but forgot to do. On account of the interesting book I am reading I forget to get off the bus and as a result I miss an important meeting. The reading of the interesting book will surely be accepted as an explanation but not as an excuse. I may be told that I could and should have taken some measures to prevent what happened.

Am I responsible for my desires, propensities, wishes and wants? I may happen to like what I am supposed to dislike and to be disgusted with what I ought to appreciate. Nobody would say that I have decided to like or to dislike, to be disgusted with or to appreciate, etc. I do not decide but I discover that I dislike or appreciate certain things. Consequently, I am not held responsible for the desires and propensities I may have at any given moment—not more than I am held responsible for the blushing of my face in a situation of embarrassment.

But desires and propensities may change and I may even know how to change some of them. While I am not responsible for the desires I actually have I may be held responsible that I have not found occasion to change or modify certain desires or propensities. I can learn to appreciate good music and classical literature and I can learn to appreciate honesty and unselfishness. As I may decide to undertake such a learning process or decide not to undertake it I am subject to blame or praise.

If we say about a person that he is repenting what he has done, what exactly are we saying? We are first of all saying that he decided to do the thing he is now repenting he did. The athlete did not decide not to run the 100-yard faster than he did; in fact he ran as fast as he could and it would be nonsense then, if he later said that he repents that he did not run faster than he in fact did. We are next saying that if he could do it all over again he would decide otherwise than he in fact did decide. He would do so because he now has some information and greater knowledge than he had when he decided. Thus, the businessman now repents that he did not sell and the doctor repents that he operated; the dishonest man now

wishes he had told the truth either because he now sees that it would have furthered his interest better or because he now sees that his self-respect suffers, etc. In short, the person who repents wishes he had not done what he in fact did, but this does not imply that he thinks that if he really could do it all over again he would—with all circumstances the same as the first time—act the way he now wishes he had acted. If he thinks so, he is mistaken; he would in fact act exactly as he did the first time.

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## VII.—DISCUSSIONS

### ON DESCRIBING DESCRIBING

A recent article in this journal<sup>1</sup> carries implications that should have been, though there is no evidence that they were, very disquieting to its authors. For instance, it implies that the following passages are not at all properly described as descriptions, that it is simply absurd on all sorts of grounds so to describe them, and indeed that we *must* not do so.

1. "Eagle Harbor is a bay extending 2 miles into the shore east of Eagle Bluff; at its head is Ephraim post office. The head of this bay is shallow for 7/8 mile out, and there are some 16- and 17-foot spots in the deep area farther out" (U.S. Lake Survey).

2. "Dunwood House was the largest and most lucrative of the boarding-houses. It stood almost opposite the school buildings. Originally it had been a villa residence—a red-brick villa, covered with creepers and crowned with terra-cotta dragons . . . A huge new building, replete with every convenience, was stuck on to its right flank" (E. M. Forster, *The Longest Journey*).

The fundamental ground on which our authors would be impelled to deny that the passages quoted are descriptions is simply that they are linguistic expressions, not actions (*loc. cit.* p. 14). To the present writer this seems just a bit high-handed, even if we use the verb-form rather than the noun, and say that these quotations "describe" Eagle Harbour and Dunwood House, respectively, rather than that they are "descriptions" of these subjects. But it should be stressed that our authors are committed to a rejection of the latter, equally with the former, mode of speaking (*ibid.* pp. 13-14) and, indeed, that without just this commitment their whole paper would be pointless. But why shouldn't sentences or paragraphs, even though they are not acts, be described as "descriptions" or even, for that matter, as "describings"? Certainly I would want, quite apart from philosophic predilections, to call 1 and 2 "descriptions", and I think that other people, and indeed that everyone who is at home in the English language, would also. But the really upsetting aspect of our authors' contention is not so much its claim that people never *do* talk this way, as its requirement that they *must* not. It is really this dictation to us concerning how we may and may not properly use 'describe' and its derivatives that bothers me, especially coming from authors who ostensibly are defenders of ordinary usage as against all varieties of autocratic prescriptions by school philosophers. We shall attempt to analyze

<sup>1</sup> *On Describing*, by S. E. Toulmin and K. Baier, January 1952, vol. lxi, pp. 13-38.

this queer behaviour in a moment ; first, however, I propose to indicate how completely and devastatingly absurd it must be to call 1 and 2 descriptions if we are to follow the path charted in the article under discussion.

Since descriptions, in this mode of talking, are actions, they clearly require an actor, specifically an author (p. 14). How ridiculous, then one must be to call a group of sentences a "description" when he doesn't know even who wrote them ! Yet this is my unfortunate state, and that, I venture to surmise, of all my readers, as regards 1, and perhaps that of some of them in reference to 2, until, at least, they looked at the designation of source.

But a knowledge of the authorship of a passage is obviously insufficient warrant for describing it as a description ; it is also "necessary to pay attention . . . to the circumstances in which the author wrote or published the passage" (p. 14). Unless it should happen that Mr. Forster's eye fell upon 2 *in the present context* and this reminded him of the circumstances of his original writing of it (an hypothesis whose probability is perhaps not quite zero), it would be extremely unlikely that anyone reading 2 would be justified, in the usage under criticism, to call it a "description". And this is also the case, only more so, with reference to 1.

To continue : describings must be to someone, as well as by someone (p. 16). It must, therefore, be simply incongruous to speak of a series of expressions as a description unless one knows that their author had an audience and who this audience was. Since I cannot be sure that I am not just "soliloquizing" in the present paper (and of course soliloquizing is to be sharply distinguished from describing), I am cut off, on this basis, from the right to designate 1 and 2, in the present context, as "descriptions". But what of them in their original settings ? Well, I really don't know just who reads the U.S. Lake Survey Bulletins : I suppose captains of lake steamers and perhaps a few yachtmen, but I don't *know* of anyone, besides myself, who has read the passage I have labelled '1'. And I am very nearly in the same predicament concerning 2. If any of my readers are better off I should be happy to hear from them.

As to the subject of description, we are told that it must not be a fact : "a fact . . . cannot be described : it can only be stated" (p. 17). On this score I would presume we must rule out 1 as descriptive—it is rather a series of statements of fact. We need not bother to ask whether 2 would meet this test for it is immediately excluded by the next one, which is that "the thing described must be what it is held out as being" so that a novelist who portrays a fictional character, is "only 'pretending to describe' someone" (p. 17). Now I would conjecture that Dunwood House, and indeed Sawston School, however "true to" English boarding houses and public schools, are only fictional entities and thus on this account that 2 cannot be a description.



Once more, to have a description, the speaker must be in a better position to speak of the subject than his audience (p. 18). This might not prohibit most of my readers from treating 1 as a description, but I fear it debars me from so doing, since it happens that I have spent many seasons sailing on Eagle Harbour and acting as racing chairman (there is a yacht club as well as a post office at the head of the harbour), so that it just may be that I am better acquainted with Eagle Harbour than was the anonymous author of 1.

I have not attempted to give a complete list of the grounds upon which our authors would be led to deny that 1 and 2 are descriptions. Indeed, I could not do this, since they did not themselves enumerate them all: "To give an exhaustive account of the notion of a description would be a very large task . . ." (p. 24). Enough have been indicated, however, to show how perfectly preposterous it must be, if our authors are right, to describe these passages as "descriptions".

Now I submit it just is not just absolutely stupid to classify 1 and 2 as descriptions. Indeed, such a classification seems to me quite in conformity with common sense and good usage. Certainly I am willing to admit that the statement, "that the words 'describe', 'description', and 'descriptive' mark off, not a particular class of sentences or words . . . but rather a particular kind of way in which we use sentences and words" (p. 24), does agree with one somewhat common use of these terms. But these words have many uses, everyday language is flexible, it should be taken in its actual contexts, we must be sensitive to its nuances, . . . And one of these very common and I should suppose eminently respectable usages allows us to speak of 1 and 2 as "descriptions".

What has happened to our "Oxford mode", if I may so refer to it? The cautions and qualifications, the it-is-sometimes-like-this-but-sometimes-not, the advice to look for the similarities but also for the dissimilarities are suddenly thrown to the winds. We are given an enormous list of conditions (and the suggestion that there are many more) all of which *must* be satisfied or we *cannot* properly speak of something as a description. We are, in short, treated to an exhibition of linguistic authoritarianism of the first order.

I am reminded of an early phase in the development of another wing of the analytic movement when certain members of the Vienna circle told us that we must use the formal, not the material, mode of speech; that our undefined, non-logical terms must pass a meaning-test; that a large number of words, at least in very common usages, are senseless, etc., etc.

So I begin to feel suspicious. Is the "Oxford mode" beginning to show signs of atavism? Is it reverting back to type—through Ryle and the neo-Wittgensteinians to the original Wittgenstein of the *Tractatus*? Is it going back on its loyalty to common usage and setting up an ideal language? That language of course is different from the ideal language of the *Tractatus*: we hear no

more of atomic and molecular sentences, but rather of gerundives and the uses of words. The meta-talk abounds in contextual and behavioural terminology, not in syntactical jargon. Still it is like the former in being prescriptive of an ideal usage, not descriptive of actual ones.

I am not anxious to criticize our authors for this tendency; indeed I share the propensity myself, though I personally feel no urge to camouflage it. But I do wish to point it out and to suggest that it may be symptomatic of something rather deep. It has seemed to me from the beginning that the neo-Wittgensteinians and those devoted to the "Oxford mode" of analysis have tried to submerge an elementary but ineradicable distinction: that between the philosophical and the common-sensical contexts and usages of language.

It is, one must admit, exceedingly difficult to get clear on what the philosopher is trying to do when he talks and specifically when he tries to analyze language, particularly everyday language. But I think it undeniable that he has a rather distinctive sort of purpose, bound up with what have traditionally been referred to as "categories", which, though possibly at times vaguely present in every-day-ish language-usage, is not specifically characteristic of the latter. In ordinary situations, language serves a variety of purposes, mostly practical. It is one of the dominant convictions of the whole analytic movement, which I share, that the philosophic concern with categories can be furthered by some kind of articulation of forms and modes of ordinary speech. But this, it seems to me, is not for the purpose of improving one's language in ordinary situations and for every-day-ish sorts of purposes. Thus a tendency to construct an ideal language that will be *categorially* more literary than, yet based finally upon, everyday language sets in. I think this is quite legitimate until it gets out of hand, i.e. becomes quite arbitrary in its clarifications, as happened with the logical positivists at a certain stage. The recent development of the "Oxford mode" of speaking has had a salutary effect in checking this proclivity. Unfortunately it shows signs itself of similarly getting out of hand.

The philosophic job of probing toward what is difficult if not, ultimately, impossible to state clearly and with finality is not furthered by peremptory prescriptions as to the only correct use of language. An ideal language, with unexceptional rules, may be a valuable tool in the philosophic enterprise, but its relations to everyday language must be a continuing subject of investigation on the part of anyone using it, and certainly it must not be slipped in, unremarked, as just everyday usage itself.

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## ON HETEROLOGICAL PARADOXES

### 1. *Known Examples of the Heterological Paradox*

(a) The following well-known paradox is of some importance in connexion with the foundations of mathematics. An adjective is called 'autological' if it applies to itself, and 'heterological' if it does not apply to itself. Consider the statement "'heterological' is heterological". If true, 'heterological' applies to itself, and is therefore autological; if false, 'heterological' does not apply to itself, and is therefore heterological. In both cases a paradox results.

(b) It has been argued that while some adjectives are clearly autological (*e.g.* English), and others are clearly heterological (*e.g.* French), there are yet others (*e.g.* mathematical) for which the question whether they apply to themselves or not is meaningless. If this is admitted, one can argue as follows.<sup>1</sup> An adjective *W* is called 'quasi-heterological' if the statement "'*W* is *W*" is either false or meaningless. Consider the statement "'quasi-heterological' is quasi-heterological". Arguing as before, it may be seen that if true, it is false or meaningless; if meaningless, it is true; if false, it is true. The paradox still remains.

(c) On the other hand, there are classes of adjectives in a three-valued logic which are free of this type of paradox. As examples we mention the autological adjectives, and those adjectives *W*, which we shall call 'anti-logical',<sup>1</sup> for which the statement "'*W* is *W*" is neither true nor false, but is meaningless. Consider the statement "'anti-logical' is anti-logical". If true, 'anti-logical' is autological and the statement is false; if meaningless, 'anti-logical' is anti-logical, and the statement is true; if false, 'anti-logical' is heterological and the statement is false. This last case is the only one which does not lead to a contradiction, so that one can conclude that 'anti-logical' is not anti-logical.

These examples leave our present knowledge of this paradox in an unsatisfactory position, since the conditions which lead to a paradox of this kind are left uninvestigated. To clarify this situation, we give below a rather more general discussion.

### 2. *Definitions*

Let there be  $n$  truth-values numbered 1, 2, . . .  $n$ ,  $n \geq 2$ , where 1 asserts truth,  $n$  denies truth, and there are  $n - 2$  intermediate truth values. Let  $j_1, j_2, \dots, j_\alpha$  be any set of  $\alpha$  truth values

<sup>1</sup> This case has been noted by J. J. C. Smart in a paper read to the Australasian Association of Psychology and Philosophy in 1951. The author is indebted to Mr. A. Flew for drawing his attention to this paper.

( $n \geq \alpha \geq 1$ ). If  $W$  be any adjective and the statement “‘ $W$ ’ is  $W$ ” has one of these  $\alpha$  truth-values, we shall invent a new adjective  $A(j_1, j_2, \dots, j_\alpha)$  which is to apply to  $W$ , so that “‘ $W$ ’ is  $A(j_1, j_2, \dots, j_\alpha)$ ” is a true statement. By allowing  $\alpha$  and all the numbers  $j$  to range from 1 to  $n$  it can easily be seen that for an  $n$ -valued logic we have in this way defined a total of

$$\sum_{r=1}^n \frac{n!}{r!(n-r)!} = 2^n - 1$$

new adjectives. In the sequel attention will be confined to an arbitrary but fixed set of truth-values, and  $A$  will be written for  $A(j_1, j_2, \dots, j_\alpha)$ .

If a statement  $S$  has the truth-value  $t$  we shall write  $S \equiv t$ .

### 3. General Discussion

Consider the statement, to be referred to as  $S$ , “‘ $A$ ’ is  $A$ ”. One can argue from any truth-value of  $S$  to other truth-values, using only the definition of  $A$ , and making no use whatever of the actual interpretation of the  $n - 2$  intermediate truth values, in the following way :

- (a) If  $S \equiv 1$ , then  $S \equiv j_1$ , or  $S \equiv j_2$ , or . . . , or  $S \equiv j_\alpha$ .
- (b) If  $S \equiv t$ , where  $1 < t < n$ , then  $S \equiv 1$  if  $t$  is a  $j_k$ .  
and  $S \equiv n$  if  $t$  is not a  $j_k$ .

(c) If  $S \equiv n$ , then  $S$  has a truth-value other than  $j_1, j_2, \dots, j_\alpha$ . The condition for no paradox to arise is that at least one of the above three arguments shall lead from a truth-value to the possibility of the same truth-value.<sup>1</sup> Only two conditions will achieve this :

- (i) If  $j_1 = 1$ , then  $S \equiv 1$  is possible, and
- (ii) If  $j_\alpha < n$ , then  $S \equiv n$  is possible.

If only (i) is fulfilled,  $S \equiv 1$  ; if only (ii) is fulfilled,  $S \equiv n$  ; if both conditions are fulfilled,  $S \equiv 1$  or  $S \equiv n$ .

We can now enumerate the number of possible paradoxes. If  $\alpha = 1$ ,  $j_1 = j_\alpha = n$  will alone give a paradox. If  $\alpha = 2$ ,  $(n - 2)$  paradoxes can be generated by the following sets of values of  $j_1$  and  $j_2$  :

$$(j_1, j_2) = (2, n), (3, n) \dots (n - 1, n).$$

Similarly, if  $\alpha = 3$ ,  $\frac{1}{2}(n - 2)(n - 3)$  paradoxes can be generated. If  $\alpha = n - 1$ , the number of paradoxes is again 1. For  $\alpha = n$ ,

<sup>1</sup> More precisely, there shall exist a truth-value  $j$  such that  $S \equiv j$  shall lead to  $\sum_{i=1}^{i=r} S \equiv j_i$ , where for some  $i$  between 1 and  $r$   $j_i = j$ .  $\Sigma$  relates to summation by alternation.

there is no paradox. The total number of paradoxes of this kind which arise in an  $n$ -valued logic is therefore

$$\sum_{\alpha=1}^{n-1} \frac{(n-2)!}{(\alpha-1)!(n-\alpha-1)!} = 2^{n-2}.$$

#### 4. Corollaries

(a)  $\alpha \geq 1$ ,  $j_1 = 1$ , and the other truth values, if any, are intermediate. The adjective  $A$  may be called 'autological' if  $\alpha = 1$ , and 'quasi-autological' in all other cases.  $S$  is either true or false, and there is no paradox.

(b)  $\alpha \geq 1$ ,  $j_\alpha = n$ , and the other truth-values, if any, are intermediate. The adjective  $A$  may be called 'heterological' if  $\alpha = 1$ , and 'quasi-heterological' in all other cases.  $S$  always leads to a paradox.

(c)  $\alpha \geq 1$  and the  $j_\kappa$ 's denote only intermediate truth values. The adjectives  $A$  may all be called 'anti-logical'.  $S$  is always false, and there is no paradox.

(d)  $\alpha \geq 2$ ,  $j_1 = 1$ ,  $j_\alpha = n$ .  $S$  is in this case always true, and no paradox arises. If  $\alpha = 2$  the adjective  $A$  may be called 'logical', since for adjectives  $W$  which are  $A(1, n)$  the statement "' $W$ ' is  $W$ " is always either true or false and has never an intermediate truth-value.

This exhausts all possibilities, and paradoxes arise only under (b).

The above remarks are conjectural in the sense that a formalised and consistent logical system into which they can be translated has not yet been constructed.

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## VIII.—CRITICAL NOTICES

*Nature, Mind and Death.* By C. J. DUCASSE. The Open Court Publishing Company, La Salle, Illinois, 1951. Pp. xx + 514.

PROFESSOR DUCASSE's Paul Carus lectures are a distinguished addition to the series. He takes as his chief problems the relations 'between nature, mind and matter; . . . between the individual's mind and his body; and . . . the implications these relations may have as to the possibility that this mind, or some part of it, survives the death of the body' (p. 3). His discussion ranges even more widely than this programme would suggest, and he has original things to say about most of the issues he raises. He displays both analytical acumen and boldness in speculation, and his style is clear and readable. The book is divided into four Parts, the first and the last being comparatively brief. The theme of Part I is 'Philosophy: its subject matter, method and utility'. Part II ('Fundamental categories') contains a detailed analysis of the concepts of causation and substance, and an elaborate and skilful attempt to reconcile the concepts of freedom and determinism. Part III ('Nature matter and mind') is mainly concerned with the theory of perception, and Part IV with the mind-body relation and the possibility of survival. Many of the views which Ducasse develops and integrates in the first three Parts have appeared in various earlier publications. Part IV, containing the most original material, is tantalisingly brief. This may not matter to those who value this book most as a synthesis of Ducasse's thinking, but some readers will wish that space had been saved earlier to allow an expansion of Part IV. For example, Part I, despite its intrinsic interest, makes little contribution to the main programme. Ducasse's theories about philosophy do not appear to be very rigidly connected with his practice thereof. One might read the remainder of this book without guessing that he considers that the philosophers' problems are essentially semantical ones concerning the analysis of *value*-words.

Ducasse rejects the regularity analysis of causation. His main objection to it is based on the fact that we often speak of C 'causing' E, where C and E are concrete events which are, or may be, unique in history. He takes Mill's canon of the Method of Difference as 'a description of the very sort of relation which is called causation', rather than as providing 'a *sign* that a causal relation (in some other sense of the term) hiddenly exists' (pp. 106-107). His own analysis amounts to this: the causal relation is that which holds between any pair of events C and E and the 'state of affairs' S, if (i) C is the only (therefore the total) change in S during period 1, (ii) E is the only (therefore the total) change in S during period 2, and (iii) period 2 commences at the moment when period 1 ends.

In view of this, Ducasse seems entitled to his claim that the 'causal tie' is 'empirically observable' at any rate in principle, but does not seem entitled to treat 'C causes E in S' as equivalent to 'the occurrence of C in S is sufficient to (necessitates) the occurrence of E in S'. Admittedly Ducasse distinguishes 'necessity between events' from logical necessity and calls the former 'etiological'; admittedly he defines 'etiological sufficiency and necessity' in terms of what may be observed in a *single* experiment (p. 110). But in view of the latter fact, his use of 'sufficient' and 'necessary' in the sequel seems misleading. He argues later (ch. 9) that we know the principles of the Universality and Uniformity of Causation on the grounds that they follow from his definitions of 'causation' and 'etiotologically sufficient'; yet he uses these principles as if they were synthetic *a priori*, e.g. in rebutting the possibility of non-deterministic analyses of 'free-will' (ch. 11). The concept of substance is analysed in terms of properties. By 'properties' Ducasse means causal capacities. To say that a certain kind of substance exists in a certain place is to say that one or more of the capacities definitive of such a substance is being 'exercised' there, i.e. that events of the kinds definitive of these capacities are occurring there (p. 167).

Ducasse opens Part III by discussing and rejecting 'radical' (as distinct from 'methodological') behaviourism. His main argument is based on an appeal to ordinary language. 'What radical behaviourism chooses to call "mind" is something which in ordinary English would be called "*the behaviour peculiar to bodies that have minds*"' (p. 230). Behaviouristic definitions of 'mind' do not, he argues, represent a factual discovery, but only 'a deep-seated resolution to make every term of the language used by natural science . . . denote at any cost something in the perceptually public world' (pp. 234-5). And here he makes use of a distinction drawn earlier (ch. 6) between an *ontological hypothesis*, which is verifiable (e.g. the atomic theory), and an *ontological position*, i.e. 'a rule one adopts or tacitly proceeds under, as to what things one will regard as alone of interest, or will rank as basic or primary' or will call 'real'. No reference is made to *The Concept of Mind*.

The main features of Ducasse's theory of perception are (i) his defence of a 'connate accusative' (i.e. internal accusative) analysis of sensation, and (ii) his view that perception always involves an activity of interpretation which is normally unconscious but sometimes introspectible. Despite the subtle distinctions he draws in chapter 13, he does not seem wholly successful in removing the paradoxical air of the former thesis. He wants to maintain both that a sensible quality like *blue* is related to *sensing blue* as a kind is related to a case thereof, and that sensing is a species of knowing. Hence his conclusion that 'that which is intuited on a given occasion is the determinate nature which the very intuiting has on that occasion' (p. 294). He is not explicit in this chapter as to how he

would apply this to sensible *spatial* relations. But chapter 15 makes it clear that he is prepared to deny that visual and tactual sensations are literally spatial! A 'place intuition' is here said to be an 'emergent' from a conjunction of kinaesthetic and tactual (or visual) sensations. What this means is not very clear, and his use of the same terms, 'sensation' and 'intuition', to refer to entities which have a sensible location and to (hypothetical?) entities which are not spatial is confusing. The main conclusion of chapter 15 is that visual and tactual perception involves a transition from an interpretand, *i.e.* a sensation which is at an intuited place, to an interpretant, *i.e.* an unformulated belief that the sensation is proximately caused by a non-intuited event at a *contiguous place* (pp. 332 and 345). This raises several problems which pass unanswered. One is that Ducasse does not reconcile this conclusion with the physiologists' inference (which he too adopts in chapter 18) that the *brain* is the only material object with which a mind (normally) interacts directly. Another is that, *on his own analysis of causation*, comparatively few of one's sensations would require one to invoke physical (*i.e.* non-intuited) causes, since, *e.g.* my opening-the-eyes sensation, when I waken, would be *the* cause of my seeing-the-ceiling sensation, my turning-the-head sensation *the* cause of . . . and so on, until I saw something move or change without any immediately preceding change in my kinaesthetic (or other) sensations. That Ducasse has not realised this is indicated by his defining 'sensations' as 'intuitions whose proximate cause is not some other intuition . . . in the same mind' (p. 295), though he clearly intends 'sensations' to denote all that it usually does. Chapter 16 contains a detailed analysis of the concept of interpretation, the species where both interpretand and interpretant are 'non-discursive' (non-verbal) being distinguished from syntactical and semantical interpretation. Chapter 17 presents his case for saying that a mind has the same title as a material object to be called 'a substance'. Though some capacities (physico-physical ones) are constitutive only of material objects, other capacities (psycho-psychical ones) are so only of minds. (Physico-psychical and psycho-physical capacities are treated as being constitutive of both minds and material objects.)

Part IV contains four comparatively short chapters. In chapter 18, Ducasse concludes that it is analytically true that a mind and 'its' body interact. In chapter 19, after presenting a formidable challenge to those who base belief in survival on ethical or theological grounds, he lists the stock arguments against the possibility of survival, and indicates how he would counter these. Chapter 20 contains a slight sketch of the empirical evidence for survival and Ducasse emphasises the inconclusiveness and ambiguity of such evidence. Then, in the final chapter, he sets out to deal with the alleged difficulty of imagining any definite form of survival that would be both theoretically possible, and worth our caring about



now. It follows from Ducasse's previous conclusions that there would be no contradiction in supposing that some mental capacities (psycho-psychical ones) persist after bodily death. He now describes several forms which survival might take, namely, the persistence of (a) a single unchanging psychical state, or (b) dream-consciousness, or (c) reflective reminiscence. He adds that (b) or (c) might also involve 'adventitious images' of telepathic or clairvoyant origin, thus providing something in the nature of a perceptible environment. He then raises a surprising question as to the relation 'in space or time between that after-death material world and the material world we now know' (p. 489). He answers that the former might conceivably be some other planet or 'this earth over again'. (This seems to confirm earlier suspicions that Ducasse has not thought out the problems concerning the relation between sensible and physical space which are posed by his analysis of sensation.) There follows a detailed discussion of the theory of metempsychosis, which Ducasse, like McTaggart, seems to regard as the most interesting form that survival might take. Ingenious answers are offered to most of the main objections to this theory, but one of these seems unanswerable, namely 'that without the awareness of identity which memory provides, rebirth would not be discernably different from the death of one person followed by the birth of another' (p. 497). Ducasse, however, seems to think that he is answering this objection by saying that 'continuousness of memory, rather than preservation of a comprehensive span of memories, is what is significant for consciousness of one's identity'. But this point, important though it is, does not seem relevant here, *unless* one maintains (and Ducasse does not do so explicitly) that when young we normally recall what we have experienced when disembodied or when animating a different body. The tentative hypothesis which Ducasse finally suggests is that death is followed by a phase of disembodied existence (*e.g.* dream-consciousness), followed by rebirth in some human body whose genetic constitution is appropriate. He claims that this hypothesis 'would satisfy pretty well most of the demands which make the desire for survival so widespread' (p. 502). Would it, however, satisfy what seems to be for many people the dominant motive—the desire for reunion with their nearest and dearest?

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*Restatement of Liberty.* By PATRICK GORDON WALKER. London: Hutchinson & Co., Ltd., 1951. Pp. x + 429. £1 1s.

THIS book is ostensibly about civil liberty, or the proper bounds of the citizen's liberty and of government's powers: but most of it is in fact occupied by historical observations and by the delineation of its author's ideal political society. It has an obvious topical interest, which is enhanced by the fact that Mr. Gordon Walker, who was Secretary of State for Commonwealth Relations in Mr. Attlee's administration and at some time an Oxford don, claims that the correct answer to this question is substantially that which is implicit in the policies of the moderate element of the British Labour Party. Mr. Gordon Walker's main concern, then, is (or ought to be) to propound his answer to this question, this being his 'restatement of liberty'. But before doing so, he considers it necessary to begin by disposing of a belief which, he asserts, underlies all the opinions on this question that are current in the Western world; for until this has been done, no reader's mind will be open to consider his thesis. The first five Parts of the book are devoted almost exclusively to this task of criticism, and only Part Six (and not all of that) is concerned with the restatement of liberty. It will be convenient to discuss the two parts of the argument in this order.

The belief that Mr. Gordon Walker wishes to disprove is an alternative proposition which may be rendered: 'Either the citizen has a moral right not to be coerced by government in any respect, or government has a moral right to coerce the citizen in all respects'; or, for short, 'Either *laissez-faire* or absolutism'. He maintains that both alternatives are equally fatal to liberty in practice. Absolutism is obviously so, and *laissez-faire* is in fact no less so, since men regularly react to its excesses by embracing absolutism. Hence, what he calls 'the dilemma of liberty': 'Either *laissez-faire* or absolutism; if absolutism, then no liberty; and if *laissez-faire*, then no liberty; therefore, no liberty.' He aims to slip between its horns by disproving the alternative premiss.

Mr. Gordon Walker's approach to this task is determined by his view of the nature of political philosophy. This question of civil liberty, he says, is a typical question of political philosophy. He adds that the proper subject-matter of political philosophy is society, and that the proper method of political philosophy in investigating this subject-matter is the historical method. 'The proper study of political philosophy is society. . . . A political philosophy that is suited to its proper study must . . . use the historical method' (p. 97). He accordingly investigates the antecedents of the belief, 'Either *laissez-faire* or absolutism'. He takes the political theories of Locke and Marx for the paradigms of *laissez-faire* and absolutism respectively, and argues in Chapters IV-VIII that they have a common antecedent in Cartesian metaphysics, his account of which seems to owe much to Professor Ryle's

*Concept of Mind.* In Chapters IX-XXII he enquires into the antecedents of Cartesianism, and finds them in the characteristics of human nature in modern Western society ('Cartesian second nature'), in the nation-state, and in industrialism. He considers that these observations will suffice to loosen the grip of the belief, 'Either *laissez-faire* or absolutism', on the modern mind, so that the way is now open to him to propose his third alternative. But before proceeding to this, it will be well to criticise the argument so far.

Mr. Gordon Walker is right in saying that the question that he is examining is a question, and indeed a typical question, of political philosophy. But it follows that he is wrong in asserting that the subject-matter of political philosophy is society. For this question is about the *proper* bounds of civil liberty and governmental powers. That is, like all questions of political philosophy, it is a question about what ought to be the case; but the study of society can only answer questions about what is, was, or will be the case. To say this is not, of course, to deny that knowledge of the facts and generalisations of political science and of the other social studies is necessary to the political philosopher; but it is to deny that it is sufficient, and to point out that he needs knowledge of moral principles as well. As for the further contention that the proper method of political philosophy is historical, I observe in the first place that Mr. Gordon Walker, like others who follow Mannheim in claiming that political philosophy proceeds by disclosing 'presuppositions', tends to confuse the question, What is the *cause* (temporal antecedent) of such-and-such a belief? with the different question, What is the *reason* (logical antecedent, or premiss) for such-and-such a belief? His statement that 'Either *laissez-faire* or absolutism' derives from Cartesianism is a statement about the relation premiss/conclusion, whereas his statement that Cartesianism derives from industrialism, etc., is about the relation cause/effect. The former, but not the latter, is consistent with his assertion elsewhere that, to disprove the dilemma of liberty, 'we must get to the unstated assumptions that underlie the process of argument that lands us within the dilemma' (p. 48). To which it must be added, in the second place, that Mr. Gordon Walker is in any case mistaken in thinking that he can extirpate our supposed belief in this alternative by exhibiting either the causes which induce us to hold it or the premisses from which it is deduced. For the only proper way to destroy a belief is to prove that the proposition believed is false or improbable. And it is plain that no statement about the causes of a belief has any bearing on whether what is believed is false or improbable, so that Mr. Gordon Walker is wrong in thinking that his supposed demonstration of a causal connexion between industrialism, etc., and the belief in this alternative can have any tendency to eradicate it. No more can it be eradicated simply by exhibiting the premisses from which it is derived. It is indeed true that it

can be proved true, and so justified, by showing that its premiss is true ; but what Mr. Gordon Walker is attempting is its destruction, not its justification. Does he perhaps think that, because this alternative is deducible from Cartesianism and Cartesianism is false, therefore it must be false ?

However, when Mr. Gordon Walker at length reaches his restatement of liberty, he sets aside the historical study of society, and proceeds by stating a general moral principle and applying it to the problems of particular liberties, namely, to the liberty of private (i.e. primarily self-regarding) acts including thought and its expression, to free enterprise and free trade, and to the freedom of association. His enquiry thus takes the same sort of form as J. S. Mill's essay, *On Liberty*, as indeed it is bound to do. The inconsistency between the method practised in this Part and that professed in the preceding Parts is plain.

Mr. Gordon Walker begins by laying down the general moral principle, that 'the overruling end' of government is 'restoring rationality and liberty and fulfilling human nature' (p. 319). The ideal society in which this end is completely attained he calls 'the better society', and what this means in practice is indicated by his remark that 'the great strides towards democratic socialism made in Britain after the Second World War do in fact permit the lineaments of the better society to be perceived' (p. 402).

My first comments on this are directed towards its meaning. By 'liberty' is meant civil liberty, or the absence of governmental coercion of the citizen either by force or by the operation of fear induced through threats.<sup>1</sup> It is clear from the course of his argument that by a 'rational' person Mr. Gordon Walker means a morally good person, and by a 'fulfilled' or 'realised' person, a person who is developed, not indeed in all respects, but in respect of those human potentialities which are generally considered good, namely, physical health, intelligence and artistic sensibility. These special uses of 'rational' and 'realised' are, of course, sufficiently familiar in ethics. Mr. Gordon Walker tends not to distinguish between government's securing to the citizen these good things, and its securing to him the means to them. He writes, e.g. of moral goodness: 'The new State . . . must do all that is necessary to create and enforce the new morality of the better man' (p. 323). But this, of course, is impossible: the citizen's virtue cannot be 'created' by governmental action, nor can he be made intelligent or cultured thereby. On the whole, it seems that Mr. Gordon Walker intends the second alternative. He writes, e.g. of 'social security' that 'the main and guiding positive purpose of social

<sup>1</sup> This is compressed but, I think, correct so far as it goes, and precise enough for present purposes. For attempts at exact definitions of 'free' and discussions of the difficulties involved, see J. P. Flamenatz, *Consent, Freedom and Political Obligation*, Chapter V, and J. D. Mabbott, *The State and the Citizen*, pp. 75-76.

security . . . is to remove certain causes of fear and uncertainty that otherwise engender irrationality' (meaning, 'immorality') (p. 326). Hence, I think that Mr. Gordon Walker's first principle may be rendered : The end of government is to secure to the citizen, first, one thing that is good intrinsically, and second, certain things that are good as being means to certain other intrinsically good things. The first is liberty, and the intrinsic goods in the second class are physical well-being, virtue, intelligence and sensibility, the means to which are cheapness and plenty, 'social security' (meaning universal, compulsory, governmentally-conducted insurance of the citizen against death, old age, sickness, accident and unemployment), economic equality, education, and displays of fine art. In short, Mr. Gordon Walker, like most Social Democrats, is an Agathistic Utilitarian.

I have two criticisms of this thesis. First, I do not think that liberty is intrinsically good, whereas it is pretty clear that Mr. Gordon Walker holds that it is. I believe that liberty is good, and that constraint is bad, only on account of their effects ; which, of course, is not to deny that they may be a very great good and a very great evil respectively. My reasons for this will appear in discussing particular liberties : it will suffice for now to say that I am maintaining that liberty of thought and discussion, *e.g.* is good, not in itself, but because it is a necessary condition of progress in the arts and sciences, which is intrinsically good ; and that coercion of thought and discussion is bad, not in itself, but because it has the contrary effect ; because it causes citizens who engage in these pursuits to feel frustrated, which is intrinsically bad ; and because it debauches the characters of those who exercise such coercion, this being another intrinsically bad consequence.<sup>1</sup> Second, Mr. Gordon Walker provides no satisfactory rule for adjudicating between liberty and the good things of the second class when conflict arises, as it easily may. Consider, *e.g.* governmental compulsion of parents to have their children educated, or treated when sick. This is required by the ends, intelligence and physical well-being, but violates the end, liberty. Naturally, Mr. Gordon Walker favours these particular infractions of liberty ; but he does not provide a reason for doing so. The nearest approach to a rule of adjustment that he provides is his statements that government must not restrict liberty so as to 'reduce the quantum of spontaneous authority in society' or 'lower respect for the authority of the State' (p. 321). But this will not do. For instance, the first maxim, if applied to the U.S.A. to-day, would preclude any further governmental restriction of free enterprise and free trade beyond what at present exists : yet it is clear from what Mr. Gordon Walker says elsewhere that he thinks that there ought to be a substantial diminution of these liberties. Again, the second

<sup>1</sup> Cf. Plamenatz, *op. cit.* Chapter VI, and Mabbott, *op. cit.* pp. 61-62 and 76.

maxim is compatible with the complete absence of liberty. Thus, if it is the case, as it may be, that the assumption of absolute power by the Russian government has not diminished the citizens' respect for it but increased it, then on this maxim there is no case for any liberty in Russia : yet it is clear that Mr. Gordon Walker holds that there ought to be some liberty in any State.

The first specific liberty that Mr. Gordon Walker considers is that of private action. His position, so far as it can be gathered from his too brief discussion, is as follows : Citizens possess an absolute moral right against government to absolute freedom of private action. 'The guarantee of their privacy is that it is the absolute right of all, including those who use it inadequately or ill' (p. 322). The reason is that government's securing them in such liberty is a necessary condition of its realising its ends of securing to them liberty and the means to virtue. 'The member of the better society cannot be rational (*i.e.* virtuous) and free unless he has a zone of privacy that is secure from the State . . .' (p. 321).

By 'private actions' Mr. Gordon Walker seems to intend actions which are primarily self-regarding, otherwise, which do not harm others. However, his statements on the point are less clear than they might be. He writes, *e.g.* 'The right to privacy must include the right to use one's privacy wrongly and to the detriment of oneself and one's society . . .' (p. 339, my italics). But an act to the detriment of society cannot be a private act, as that term is generally understood by Mr. Gordon Walker, since he generally uses it, as I have said, as equivalent to an act which does not harm others. Under the rubric 'private actions' he includes explicitly thought and its expression, and implicitly what Mill calls 'tastes and pursuits' :<sup>1</sup> his position is, therefore, that citizens have an absolute right against government to absolute liberty in these respects.

The assertion that the citizen has an *absolute* moral right to absolute liberty of private action is surely inconsistent with Mr. Gordon Walker's own premisses. For these require him to say that the citizen possesses this right only because government always in fact defeats its proper end by exercising any compulsion upon him in the sphere of his private affairs. But if this fact should alter, he would cease to possess this right : so that it cannot be an absolute or unconditional right. It is also incorrect to include *all* expressions of thought and all tastes and pursuits under the heading of private actions : only those that do not harm others can correctly be so included, as Mill recognises.<sup>2</sup> Instances of expressions of thought and of tastes and pursuits which do harm others are libels and very fast driving. Further, it is surely not true that the citizen cannot be virtuous or free unless he is *absolutely* free in respect of his private actions. What is true is that he cannot properly be said to

<sup>1</sup> *On Liberty*, Everyman edition, p. 75.

<sup>2</sup> *Op. cit.* p. 75.

be free at all unless he has some, and indeed a substantial amount of, freedom in this respect, and that he cannot in fact be virtuous unless he has some liberty to think about and discuss moral questions and to do or refrain from certain acts, such as giving to charity. Finally, there remains the central question: Is it really true that the citizen has a right never to be coerced by government in respect of actions (if any) which harm himself alone? Suppose, e.g. that I have a taste for eating opium and that my indulging it harms no other. Personally, I am inclined to agree with Mr. Mabbott, that a law restraining me from doing so is nevertheless not wrong.<sup>1</sup>

In Chapter XXIV Mr. Gordon Walker considers free enterprise and free trade. The actions comprised under these heads are not, of course, self-regarding, since a man may harm another by competing against him, and are not therefore such as to fall automatically outside the scope of governmental coercion. As might be expected, Mr. Gordon Walker contends that both of these freedoms should be substantially restricted: certain enterprises ought to be conducted exclusively by government or its agencies (otherwise, be 'nationalised'), and free trade ought to be limited by 'planning', meaning, by 'control of credit and the rate of interest' and 'regulation of capital investment and foreign trade' (p. 335). Yet they ought to exist to some extent, because the end of government requires that they should. 'The need for non-State enterprises and concerns is deducible from the nature of the new State and the better society: it follows from the inability of the State, so long as it is true to its nature, to plan and manage the whole economy in detail' (p. 335). (By 'being true to its nature' is clearly meant 'realising its proper end', in particular, securing to the citizen some liberty.) Their precise extent ought to be determined, however, by efficiency, i.e. by the test of cheapness and plenty. 'The distinction between the private and public sectors of the economy must be a technical one dictated by the needs of the economy: not a moral one' (p. 337).

On this I comment, first, that Mr. Gordon Walker's two maxims, as stated, may conflict. Suppose that the economy of Great Britain would be most efficiently conducted if all economic affairs were conducted exclusively by government. In this case, the second maxim requires that this is what should be done, whereas the first requires that it should not. What Mr. Gordon Walker's premisses require him to say, I think, is that the extent of free enterprise and free trade ought to be determined by both efficiency and liberty considered jointly. Thus, in this hypothetical case, it would have to be decided at what point the good of a further increase of efficiency was outweighed by the evil of a further diminution of liberty. So stated, the argument seems to me to be correct: my only disagreement with Mr. Gordon Walker would then be, once

<sup>1</sup> *Op. cit.* p. 61.



again, about whether liberty is good as a means or as an end. For, whereas he asserts the latter, I should maintain that the reason for conserving some degree of free enterprise and free trade, even at the cost of efficiency, is the bad consequences of 'total planning'; namely, its probable bad effects on the characters of both rulers and ruled, and the risk of its abuse inherent in creating so great a concentration of power.<sup>1</sup>

Chapters XXVI and XXVII are concerned with the right of freedom of association. Actions under this head, again, evidently do not fall under the principle of self-regarding actions, since an association may well be harmful to others (*e.g.* a secret society of assassins), and hence also do not automatically fall outside the proper scope of governmental coercion. Mr. Gordon Walker argues, therefore, that '... society must possess the right to limit the freedom of organisation' (p. 360). Specifically, he contends that 'The new State ... need properly concern itself only with those actions of powerful organisations that may harm society or other organisations' (p. 363). An example is a general strike by trade unions. But insofar as their combinations do not harm others, citizens have a right of free combination, since this is immediately derivative from their right to do what does not harm others, otherwise their 'right to privacy'. 'The right to associate with like-minded fellows cannot be infringed without trespassing upon the zones of privacy that the new State must guard and secure' (p. 349).

The last contention is the same as Mill's.<sup>2</sup> The missing link in the argument is provided by the important concept of free contract. Association essentially involves individuals contracting together to promote a purpose collectively, and the right of A and B to contract freely with each other is generally regarded as immediately derivative from their right to freedom of tastes and pursuits; so that, if A and B have a right to be free to do individually something that does not harm others, then they have a right to contract with each other to do that thing collectively. However, this is clearly open to the same objection as has been urged against the supposed right to individual liberty in self-regarding actions. For if, as I have suggested, government has a right to prohibit me from eating opium even though my doing so harms no other, then it also has the right to prohibit me and Thomas from forming an association for the purpose of eating opium together on wet Sunday afternoons, even though our doing so harms no other. Further, Mr. Gordon Walker seems not to distinguish between prohibiting an association and prohibiting an association from doing certain acts. But it is clearly one thing to prohibit trade unions from engaging in a general strike, and another thing to prohibit trade unions; and it is the latter, not the former, which involves the right of free association. What

<sup>1</sup> These are substantially the reasons that Mill gives, *op. cit.* pp. 164-165.

<sup>2</sup> *Op. cit.* p. 75.



Mr. Gordon Walker ought to conclude, I think, is that government has the right (and the duty) to prohibit all associations which are essentially harmful to others. Whether a combination is essentially harmful to others is decided by its purposes, as explicitly defined by its articles of association or implicitly revealed by the general course of its actions. The point is amply illustrated by the changes of opinion and law in England during the nineteenth century concerning trade unions. The Utilitarians of the Right held that trade unions were essentially harmful to others, since they were combinations in restraint of free trade in labour and hence of free contract; but free contract was considered by them to be an essential means to maximising the general happiness. Hence, they argued, men (*e.g.* employees A and B) have no right to be free to contract with each other to infringe other men's (*e.g.* employer C and employee D) right to freedom of contract. A similar argument was of course directed against combinations of employers. The Utilitarians of the Left (notably, J. S. Mill) maintained on the contrary that trade unions were not essentially harmful to others, since the general happiness would in fact be better promoted by free 'collective bargaining' in labour than by free individual contract. They recognised, nevertheless, that certain classes of acts by trade unions were harmful to others (*e.g.* intimidation of non-members), and ought to be prohibited accordingly. This conflict of opinion is clearly reflected in English legislation of the nineteenth century.<sup>1</sup>

My last criticism on this point is that Mr. Gordon Walker discusses freedom of association almost exclusively in terms of trade unions, and barely mentions churches and political parties and other important combinations. But this, in a book of over 400 pages devoted to liberty, is unacceptable. An adequate discussion of the great and thorny topic of religious liberty (or toleration) alone might fill a volume. And so far as topical importance goes, the issues raised by such events as the imprisonment of Niemöller and Mindszenty and by the proposed banning of the U.S. Communist Party rank at least equally with those implicit in the Osborne Judgment.

Finally, in Chapter XXVIII, Mr. Gordon Walker discusses what he calls government's 'powers of persuasion'. He distinguishes those functions of government which involve coercion (and hence liberty) from those which involve persuasion, and lays down the general principle that government has the right and the duty of persuading (otherwise, employing propaganda), as distinct from coercing, insofar as this conduces to its end as previously defined. He writes, *e.g.*: "It is the duty of the new State to urge individuals and organizations to make the savings that the economy needs or to urge trade unions to limit wage demands and companies to limit

<sup>1</sup> Cf. A. V. Dicey, *Law and Public Opinion in England during the Nineteenth Century*, especially pp. 149-157.

dividends or to engage in forms of economic activity that fit into the general pattern of the economy. Even where the State may properly use direct coercive powers it must also use propaganda where this can help to achieve the desired results and reduce the need for coercion and punishment" (p. 370). He adds the qualification that government must not monopolise any of the means of persuasion (e.g. the schools and broadcasting facilities), else there is a violation of the citizen's right to freedom of expression. Mr. Gordon Walker also argues that two further proper exercises of government's powers of persuasion are found in the spheres of culture and education. As to the first, he asserts that government ought to provide funds for the supply of displays of fine art beyond what there exists an effective demand for (e.g. grand opera), but that it ought itself neither to supply the displays nor to influence them. As to the second, he asserts similarly that government ought to provide funds for the supply of education beyond what there exists an effective demand for, but that it ought itself neither to supply the education, still less monopolise its supply, nor to influence it.

Mr. Gordon Walker's distinction between the coercive and the persuasive functions of government recalls J. S. Mill's distinction between the authoritative and the unauthoritative interventions of government.<sup>1</sup> But there is a significant difference; for whereas Mill argues for government's right of 'giving advice and promulgating information', Mr. Gordon Walker argues for government's right and duty of persuading by propaganda. Persuading differs from informing and advising; in particular, it comes closer to coercing. I submit that there is a case against government's possessing a right to persuade, for the following reasons. First, it is a fundamental principle of sound policy that the citizen should know just where he stands in his dealings with government. But when government seeks to persuade, this rule is broken. Mr. Gordon Walker's own examples illustrate the point well. When a trade union leader or a company chairman is *urged* by an officer of government not to demand an increase of wages or to increase a dividend, he does not know where he stands. Is he being coerced by threats, or is he not? That is, is his liberty being infringed, or is it not? The practice seems to me bad policy for these further reasons. Second, it contrives to shift the burden of a responsibility that properly belongs to government on to the shoulders of the citizen. For the responsibility for the economy (and, generally, for the interests of the State) as a whole is government's, whereas the responsibility of a union leader or a company chairman is to his union or company. Third and consequently, when the persuasion succeeds, government is enabled to claim for itself achievements in a manner that is not quite candid: 'Without infringing the liberties of free contract and free trade by compulsory wage and

<sup>1</sup> *Principles of Political Economy*, Book V, Chapter xi, § 1.

dividend restrictions, my administration has nevertheless been entirely successful in holding inflation in check.' These substantial evils seem to me to outweigh the consideration that might be pleaded on the other side, that persuasion is a lesser evil than coercion. Hence, I suggest that government's actions should be guided by the maxim: Government ought to coerce rather than to persuade, and to resort to persuasion only when coercion is impracticable. The qualification is illustrated by the case of a government which, in a time of war when the railway system is overburdened, dissuades citizens from consuming that service by advertising the question, 'Is your journey really necessary?' For it is plainly impracticable to prevent by force or threats all 'unnecessary' journeys. At all events, the question is interesting, and deserving of more attention than it has yet received, on account of the rapid and extensive growth of the practice.

Moreover, Mr. Gordon Walker's justification of government's interference with the supply of artistic displays and education, on the ground that they are instances of the proper exercise of its persuasive function, must be categorically rejected. The chief reason is that, whatever the proper ends of artistic performances and of education may be, they are certainly not persuasion. Further, the contention that they are is inconsistent with Mr. Gordon Walker's own statements previously noticed, that government ought not to influence the education or culture provided; for if this is to be so, how can government use, say, Oxford University or the Covent Garden Opera as instruments of persuasion? Again, it is surely accepted in civilised States that even the appearance of anything of the kind must be assiduously avoided, as witness, *e.g.* the nature of the institutional machinery by which government pays moneys to the British Universities. Finally, it is not strictly true that these two governmental functions do not involve coercion, and hence liberty. For they have to be paid for by taxation, which necessarily involves restricting the citizen's freedom 'to do what he likes with his own' (otherwise, his right of private property): they are therefore in a different case from governmental operation of postal or air-transport services, which are in principle strictly economic enterprises. (This is also true of many governmental acts which genuinely are persuasive. Posters urging citizens to work more or travel less must be paid for out of taxes, so that Mr. Gordon Walker's division of the functions of government into coercive and persuasive is not correct, since the classes are not mutually exclusive. The presence of this element of coercion constitutes another objection to government's exercising persuasion additional to those detailed above.) I suggest then, that governmental interference with the supply of education and culture is properly justified on Mr. Gordon Walker's premisses by, first, admitting that it is coercive and involves liberty; second, observing that the coercion is consistent with the privacy principle,

since others are harmed if they are denied education and culture ; and third, arguing that the intrinsic goodness of citizens being educated and cultivated outweighs the badness of the consequences of their being compelled to pay for the necessary means to these ends.

I have noticed the following misprints : p. 75, para. 5, for 'epistemology' read 'epistimology' ; p. 79, para. 5, for 'Goodwin' read 'Godwin' ; p. 85, para. 2, for 'Toqueville' read 'Tocqueville' ; p. 87, para. 5, for 'Treitscke' read 'Treitschke' ; p. 166, para. 4, for 'Amercianism' read 'Americanism' ; p. 166, footnote 1, for 'Fendalism' read 'Feudalism' ; p. 333, para. 2, for 'objections' read 'objectives' ; p. 400, para. 4, for 'on his cultural plane' read 'on the cultural plane'.

J. P. DE C. DAY.

*Structure, Method and Meaning.* Essays in honour of H. M. Sheffer. With a foreword by FELIX FRANKFURTER. Edited by PAUL HENLE, HORACE M. KALLEN, and SUSANNE K. LANGER, 1951. The Liberal Arts Press, New York. Pp. xvi + 306. \$4.50.

THIS book contains twenty essays on subjects ranging from pure mathematics and science to art and jurisprudence. While it reflects happily on the wide circles of Professor Sheffer's friends and interests, it is necessarily a difficult book to review. Some of the contributions give the impression of being rather hurriedly written, one or two on subjects which appear not to be the writer's professional interest. At least half of them cannot be properly understood without recourse to the other published works of the author, a fact witnessed by numerous self-referential footnotes and perhaps due to a desire to produce a thoroughly representative piece of work. I shall discuss only what seem to me the most philosophically interesting papers. The book is divided into three sections under the headings of the title. In the first section which is mostly devoted to symbolic logic W. V. Quine shows briefly that elementary number theory can be founded on a three term primitive such that if it holds between three natural numbers  $x, y, z$ , then the ordered pair  $x : y$  has fewer than  $z$  precursors in some pre-arranged progression of ordered pairs of natural numbers. J. W. Miller formulates a series of rules like the rules of the syllogism but designed not to test the categorical syllogism but the other syllogisms, sorites, and immediate inferences of the traditional logic, making explicit the existential presuppositions of such inferences. P. Henle sketches a generalization of Boolean algebra in which the law of double negation is replaced by laws of treble, quadruple, and in-general,  $n$ -tuple negation.

Alonzo Church in an elaboration of an address to the association for symbolic logic discusses the axiomatic treatment of the logic of

sense and denotation. Hitherto his views have been available only in an abstract of the address, and this article is particularly welcome on that account. For reasons which were explained in an article in *Analysis* in 1950, Church regards any essentially meta-theoretic analysis of the notion of sense as open to fatal objections when applied to statements of belief and assertion. His systems make use of no hierarchy of metalanguages although they contain the names of concepts. The paper is difficult for the reader only cursorily acquainted with mathematical logic since it employs the notation of the author's *Formulation of the simple theory of types* and is not easily understood without reference to it. Church starts from Frege's distinction between the sense and denotation of a name, and construes every well-formed expression in which no variables occur unbound as a name which denotes an object and has a sense. The sense of any name which denotes an object is called by Church a concept of that object. In particular, propositions are concepts of truth values, and hence the senses of certain sentences. Since no expressions are mentioned in the system no concept can be identified in the system as the sense of a particular name. This could of course be done if the individuals of the system were construed as expressions, although any explicit identification would of course be factual and not analytic. In spite of this, for every name in the system there is another name (constructed from the first name by a transformation of type symbols) which is in the intended interpretation construed as denoting the sense of the first name, and the names constructed in this way are the only constants which denote concepts. The axioms provide for each such primitive constant that it is a concept of the appropriate object by means of a primitive symbol ' $\Delta$ ' such that  $\Delta AB$  holds if and only if B is a concept of A. In general Church accepts literally much of Frege's doctrine. He differs from Frege in not using an assertion sign and in not discussing possible reasons for its use. He also employs an ingenious and economical identification of the concept of a function with a certain uniquely specified function of concepts. This is done as follows. The system starts with two type symbols ' $L$ ' and ' $O$ ' representing the types of whatever individuals are chosen and of truth-values respectively. A concept of an individual is of type  $L_1$ , a concept of a truth-value is of type  $O_1$ . A concept of a concept of an individual is of type  $L_2$ , and so on. A function from arguments of type  $\beta$  to arguments of type  $\alpha$  is of type  $\alpha\beta$ . If there is such a function denoted by an expression ' $F_{\alpha\beta}$ ' the sense of this expression is identified with a function of type  $\alpha_1\beta_1$  such that its value for the sense of a name ' $x_\beta$ ' which denotes something of type  $\beta$  is the sense of the name ' $F_{\alpha\beta x_\beta}$ ' which of course denotes something of type  $\alpha$ . Thus for any well-formed formula without freely occurring variables a formula obtained from it by raising the subscripts of all the type symbols, will be of the right type to denote a concept of the object denoted

by the first formula, and is in fact construed as denoting that concept which is the sense of the first formula.

As in the abstract of the earlier paper, Church distinguishes various ways of deciding when two names shall have the same sense. In this paper he devotes his attention primarily to a system which assigns the same sense to 'A' & 'B' if and only if ' $A = B$ ' is logically valid: but he regards this as a preliminary to a more complicated system which makes the senses of 'A' and 'B' different whenever the extensional character of the system permits this, or alternatively, to a system similar, except for its meta-theoretic character, to that of Carnap's *Meaning and Necessity*. In the principal system of this paper therefore there is a fairly simple definition of necessity in terms of sense.

The philosophical character of a modal system turns in the first place on which non-modal expressions are flagged as necessary and also on how the modal symbols themselves may be combined and permuted. The notion of logical necessity is involved in the *description* of a deductive system since the description includes transformation rules which have logical consequences. Apart from this the notion of logical necessity enters into the system only through the interpretation of the axioms as necessary, the possibilities of interpretation being of course governed by the transformation rules. This level of interpretation is of course distinct from that in which the individuals and their predicates are identified. It follows that if all the axioms are interpreted as necessary (as is the case with Church's interpretation), then the theorems will also be necessary. If it is further intended that a statement of the necessity of a theorem shall syntactically be a consequence of the axioms, then if the theorems are necessary, they are necessarily necessary. At least this will follow unless a more than systematic ambiguity in the word 'necessary' is countenanced. But having got so far one has learnt no more than was involved in the original decision to interpret the axioms as necessary.

Church defines a symbol for necessity which prefixed to the name of a proposition (*i.e.* an expression denoting the sense of a sentence) results in an expression which equates this sense with the sense of a, presumably arbitrarily chosen, analytic sentence. He then supplements his original axioms with another batch indicating that the original axioms are necessary. For this new batch a still further lot of axioms are forthcoming and this process is sketched as continuing until it becomes clear that a set of axiom schemata will cover the whole lot. He remarks that the system so constructed admits an interpretation which ignores his distinction between concept and object and so reduces to his *Simple theory of types*, and says, I think rightly, that this cannot be avoided if all the axioms are to be construed as necessary. Even so it is important to see exactly how the intended interpretation avoids this result: *i.e.* we must examine the principles governing the combinations of modal

symbols, and hence the axioms relating to the symbol ' $\Delta$ '. On Church's view a sentence which denotes truth expresses a necessary proposition if the statement that that proposition (*i.e.* sense of that sentence) is a concept of truth is analytic. A statement  $\Delta AB$  may be true factually or analytically. If it is true analytically or as I shall say if B necessarily determines A and A is the truth-value truth then B is a necessary proposition. The point of reiterating modal symbols in this system is to cover the possibility of referring to a proposition by means of a description which only contingently denotes that proposition. To say that something is necessary is to say something about a proposition, *i.e.* the sense of a sentence or concept of a truth value. A proposition has the type  $O_1$ . To say that something is necessary is to say something like  $p_{01} =_0 T_{01}$ , where ' $T_{01}$ ' denotes the sense of the chosen analytic sentence, and the subscript on the ' $=_0$ ' indicates that this sign refers to a function from arguments to truth values. To say that it is necessary that something is necessary is to say that the sense of some sentence like ' $p_{01} =_0 T_{01}$ ' is the same as the sense of the chosen analytic sentence. But the sense of a sentence like ' $p_{01} =_0 T_{01}$ ' is denoted by an expression which includes a symbol like ' $p_{02}$ ', since the senses of expressions have in general higher subscripts than the expressions themselves, and as a matter of interpretation the sense of a particular expression is achieved by raising the subscripts of that particular expression. Thus the question whether the sense of a sentence like ' $p_{01} =_0 T_{01}$ ' is analytic is the question whether the concept denoted by the symbol ' $p_{02}$ ' (which is the symbol which occurs in the expression interpreted as denoting the sense of ' $p_{01} = T_{01}$ ') is a concept which necessarily determines an analytic proposition. If  $p_{02}$  is such a concept then  $p_{02} =_0 T_{02}$  and vice-versa. We thus reach Church's principle of double necessity which asserts that if and only if  $p_{02} =_0 T_{02}$  is the sense of a sentence like ' $p_{01} =_0 T_{01}$ ' analytic, where  $p_{02}$  is that concept of  $p_{01}$  which as a matter of interpretation is the sense of the expression ' $p_{01}$ ' and consequently a component in the sense of the expression ' $p_{01} =_0 T_{01}$ '. Symbolically  $(p_{02} =_{01} T_{02}) =_0 T_{01}$ , if and only if  $p_{02} =_0 T_{02}$ , where the expression in brackets is interpreted as denoting the sense of ' $p_{01} =_0 T_{01}$ '.

It can thus be seen that since theorems in the system which assert that something is a concept of something else are intended as analytic, the explanation of the intended interpretation of the system assumes that in some contexts if a reference is made to the sense of an expression by some sort of autonymous device, the concept to which reference is thus made necessarily determines the object to which the original expression referred. It is not clear whether Church thinks the English language has other ways of referring to such concepts. In an example Church explains that whereas it is no doubt necessary that it is necessary that *every object has some property or other*, it is true but not necessary that



the proposition mentioned on lines 27-28 of page 272 of Lewis and Langford's *Symbolic Logic* is necessary. For although this proposition is the proposition italicised above, yet this is only a factual identity. It looks as though a given autonymous use of a sentence may sometimes have to be construed as denoting concepts of different orders according to the width of the context in which it is considered.

Church's primary concern seems to be the guaranteeing of consistency by means of an adequate theory of types and this is a mathematician's interest unaffected by the fact that we are ordinarily able to locate contingent facts of the type cited above (*i.e.* which give sense to iterated modal symbols) without recourse to necessary necessities. Philosophically his system requires that every sentence, description and name has a sense, the sense of a complex expression being a function of the senses of its component parts. This chain-into-links analysis would of course also be important in the proposed alternative system which is to make partial use of Carnap's method. However, the fact that differences between the senses of two descriptions must be traced to differences between the component parts of the two descriptive phrases does not obviously imply that we must attribute a sense to each separately significant part of a descriptive phrase. This assumption, which is connected with the notion of a concept necessarily determining an object, involves Church, when interpreting his system, in saying that his symbol 'C' is intended not only to denote material implication, but also to have the sense of material implication, a qualification which is not easy to understand. This latter difficulty is bound to reappear when a narrower meaning is attached to the notion of sense as in either of the alternative systems. It is also likely that some sort of redundancy in symbols for necessity would appear since, as Church points out, senses in the system discussed here could perhaps be defined as classes of senses in the alternative systems. A compact deductive treatment of synonymy can only characterize certain syntactical transformations as those which do not affect the sense of an expression. This is not very likely to yield a definition of proposition which will count as the content of a belief. Further, in practice, it does not seem obvious that synonymy is transitive; questions of sameness of meaning turn largely on the context of discussion. Such contexts embody empirical factors, and this throws some doubt on the view that the analytic logic of sense can carry one very far. One can concede that synonymy is relative to context without having to admit that it is never *really* true that two expressions mean the same. These considerations cannot be brushed aside on the ground that these words are vague in their ordinary use. There is no point in pretending that the system has an application unless it can be shown what sort of application is intended. Axiomatic treatment of logic has been philosophically interesting often because it shows



up the character of natural languages by failing to exhibit accurately their structure.

C. I. Lewis in a shorter paper starts with a more philosophical sketch of what may be involved in applying the terms intension and extension to propositions. He connects this application with Sheffer's use of the term 'ascriptive' denoting roughly the content of a proposition regardless of its being asserted, denied or questioned. When so considered, says Lewis, a proposition turns out to be a kind of term whose extension is the actual world in case it is true and nothing in case it is false. A proposition signifies a state of affairs which Lewis regards not as a space-time part of the world, but as an attribute of the world. The rest of his paper is devoted to a sketch of a system related to the functional calculus much as the calculus of classes is related to the propositional calculus. H. S. Leonard shows how the two truth values T and F are adequate to constructing a type of truth table involving modal symbols, provided that in determining the truth value of an expression involving a modal symbol one is not restricted to the consideration of truth values of the row containing the truth value which is to be calculated. The method is eliminative. A modal expression is allowed to assume both values except when this is forbidden by a complex restriction referring to the whole of the truth-table so far constructed. This in turn involves that the headings of the table be arranged in a certain order. Leonard considers different restrictions which turn on the acceptance or non-acceptance of Becker's postulate.

In the second section C. J. Ducasse in the longest essay in the book sympathetically defends Bacon's account of scientific method from criticisms and misinterpretations mainly due to Ellis. Ducasse considers the doctrine of method of the first book of the *Novum Organum* admirable, though he admits that no amount of exegesis can lead one to say as much of the second book. Amongst one or two vague and grandiose essays on science and philosophy in general Marvin Farber is refreshingly practical in turning to history "to answer the question regarding the nature and function of philosophy; but for reasons that are apparent at once that can only be done imperfectly". Even if one disagrees with this approach it does at least provide a check on the naturally wayward character of *a priori* accounts of philosophical method, and also breeds a certain tolerance of philosophies other than one's own.

Mrs. S. K. Langer contributes a paper called *Abstraction in science and in art* which is perhaps the most provocative and interesting paper in the book apart from the first section. It is a difficult paper to assess because of its use of abstract terminology. The view presented has much in common with her *Philosophy in a new key*. She points some contrasts between form in art and logic, while maintaining that there is an underlying meaning of the word 'form', according to which form is not accidentally but essentially

involved in both art and logic. The argument runs roughly as follows. In art and in logic abstraction is the recognition of form : all symbolization rests on the recognition of congruent forms and hence on abstraction. (Sometimes Mrs. Langer seems to hold the narrower position that any process of abstraction is a process of symbol making.) Lastly, she asserts that a work of art is a symbol and so involves abstraction and the recognition of form. It is important to see in what sense she thinks a work of art is a symbol, and in what sense all symbolization rests on a recognition of congruent forms. Taking the second point first, this seems to mean not that any two things symbolized by the same symbol must have a congruent form, but something nearer to Wittgenstein's view in the *Tractatus* that the form of the proposition must match the form of the fact symbolized. At least Mrs. Langer cites one-one correspondence of name and thing as an example of the recognition of congruent forms. In a work of art she seems to hold that the complex form symbolizes the dynamics of subjective experience, "the pattern of vitality, sentience, feeling and emotion". This symbolization is not by the conventional assignment of symbolized to symbol, rather the form of a work of art is significant simply by virtue of its articulate character. Some remarks about the etymology of sensory predicate words suggest that she thinks that the symbolic character of a primitive language approximates to this sort of aesthetic symbolization of feelings. From this point various distinctions between form in art and logic are made. Abstraction is not used in art for purposes of generalization, nor are the artist's symbols systematically combinable as in language. The interest of such contrasts turns however on the force of the original identification of form in art and logic. Mrs. Langer's thesis amounts in fact to a theory of symbolization in which the recognition of congruent forms plays an important if not the only part. What seems to be left out of the theory is the notion of the function of the symbol as opposed to characteristics which it must have in order to function as a symbol. After all abstraction is a feature of all our experience anyway, so that until it is clearer how the function of abstraction in art resembles the function of abstraction in logic, it is not easy to see that the differences in these fields are important. One specific point that needs illuminating is why a work of art is a symbol. Other more detailed points deserve notice. The contention that the significance of a work of art is due to its form is not entirely convincing. We can admit that a temporal climax in music can match a temporal climax in emotion, but more elusive qualities do not so obviously fit this correlation. There seems to be no room for the effect of association. Mrs. Langer asserts that what is abstracted in art is form, and that its symbolic character turns on a recognition of congruent forms. ('Recognition' seems the wrong word here.) But this either expands the meaning of form to the point at which any aspect which is noticed is a form,

or else calls for an exact explanation of what is and what is not formal. This explanation is likely to be impossible in some cases on Mrs. Langer's view since she holds that some forms are not expressible and so presumably not describable in words at all. On the other hand, it is important to know in what way logical form is connected with symbolization. Mrs. Langer has not much to say about the logician's use of form, and I feel that she has here neglected the fact that from one point of view the symbols of pure logic do not symbolize anything but their own use. It is possible of course that the notion of logical form depends in the last resort on semantical explanation, but this point is not discussed at all.

F. S. C. Northrop starts from the unexceptionable premiss that the meaning of an otherwise uninterpreted expression in a deductive system can be understood only by reference to the axioms and rules of the system. On this ground he argues that it is absurd to try to analyse the meaning of any individual proposition apart from the deductive theory of which it is a part, and hence that "the meaning of ethical propositions and concepts cannot . . . be determined by the present analytical method of both the Oxford idealist and the Cambridge realistic moral philosophers, by analysing individual ethical propositions apart from the deductively formulated or formulatable philosophical assumptions of the philosopher making the analysis, or of the philosophy of the particular culture from which the sentence gets its socially communicable meaning". As it stands the conclusion does not follow from the premiss since ethical propositions are not uninterpreted expressions nor are they ordinarily items in deductively formulated theories. Northrop does not articulate the argument further, and in mentioning what he calls concepts by intuition (somewhat naïvely explained as concepts whose complete meaning can be given by something that can be immediately apprehended) he does not explain how they are related to concepts specified by the postulates of a system. It would presumably be by the development of some such account of empirical meaning that his argument should be supported. Notwithstanding the weakness of the argument there might be something in the conclusion. In the paper Northrop gives no specific examples at all of what is involved in relating an ethical proposition to the appropriate deductive theory, nor does he make it clear whether he thinks that it is words like 'enlightened' and 'prudent', or words like 'theft' and 'adultery' that should feature in an explanation of a word like 'good'. He must presumably mean something more than exhibiting the deductive relationships of a sentence like 'Jones is unprincipled', since this is largely what is involved in analysis in the sense in which he attacks it. What Northrop actually emphasizes is that an American sociologist who thinks that he can obtain an understanding of a foreign culture by a *hard-boiled* description of the *empirical* facts as he sees them, is indulging

in self-deception. He must pay attention to the conceptual apparatus of the language of that culture and notice where it differs from that of his own language. This contention is supported by references to anthropologists, notably Kluckhohn and Sorokin, and seems reasonable enough, but Northrop's view that objectivity can be achieved only by the use of deductively formulated theory to "bring out into the open the conceptual primitive concepts and postulates of that culture" seems to assume that the notions of primitive concept and postulate apply to natural languages, and also that the meaning of an expression is exhausted by its deductive relationships. The final conclusion is characteristically optimistic. Northrop believes that, since the ethical and legal norms of different societies are reflected in what he calls "the conceptualisation of empirical data" in that society, the problem of an objective ethical norm is in part at least the problem of discovering which conceptualisations have the greatest deductive fertility in accounting for all the "data given with positivistic empirical immediacy". There seems to be a trace of self-deception in this conclusion.

The last section contains an historical note about the development of William James's philosophy by R. B. Perry. It makes discouraging reading since in the space of seven pages at least fourteen philosophical tendencies are mentioned by name (experientialism, panpsychism, abstractionism, etc.) with insufficient explanation of what these words mean. F. S. Cohen in an adaptation of a paper previously published in the Yale Law Journal discusses the notion of 'field' in connexion with judicial theory. He enquires whether the 'practical know-how' that enables an experienced judge to discount the bias of the witness and the lawyer could be formulated and thus rendered more systematic and less haphazard. He argues that both in the matter of evidence and the interpretation of precedent the situation of the judge is not unlike that of the physicist confronted with the uncorrelated reports of different observers. As might be expected no very exact answer to his question emerges, although difficulties in the way of an affirmative answer are listed and discussed. The central part of Cohen's paper is partly obscured by a compressed statement of a view of his concerning the logical necessity of following precedent which does not seem relevant to the main contention of this paper.

D. C. Williams, in the last essay in the book, is concerned to defend what he calls the view of the world which treats the totality of being or facts or events as spread out eternally in the dimension of time as well as the dimension of space. Less picturesquely, he wishes to combat any denial that it is true now that there will be—or will not be—a sea-fight tomorrow. He attributes denials to both Aristotle and C. D. Broad, saying that he finds Aristotle's remarks on the matter condensed but not inscrutable. I am not convinced that Williams's paraphrase does justice to Aristotle, since with a slight hesitancy he treats the whole of the passage (18a 28-19b 4)

of the *De Interpretatione* as a continuous argument concluding with a *reductio ad absurdum*. Williams is in favour, for philosophical purposes, of dispensing with tenses, and making an independent indication of the time at or during which the action or event took place. Having done this he still wishes to say of such sentences not that they are true, but that they are true now. I do not understand what is achieved by this; the inference "true, so true now" is not pointless for Williams since he equates "a statement about the future is true now" with "the future is real", and maintains that such assertions are indifferent to views about determinism. It is not difficult to believe that saying statements about the future are true does not commit one to determinism, but one cannot be sure what is entailed by saying that such statements are true *now* until it is clear what this means. It is very natural to treat the *now* in such a sentence in the ordinary way and start looking now for something referred to in the sentence, and if this is not how the word is to be treated, then one wants to know what *is* to be understood by it. C. Hartshorne, by contrast with Williams, assumes that the spatialized view of time is discredited, and with it any view that alleges that a man is or includes the sequence of events describable in his biography. This being so he asks what is the identical referent referred to by the pronoun 'I'. Having rejected collections of events there remain on his view only separate events and abstractions. He then argues that the concrete separate event is the logical subject of its attributes, and is clearly not the identical referent. From this it follows that what is referred to by the personal pronoun must be an abstraction, in which memory and spatio-temporal relations figure, and cannot be the logical subject of which the successive biographical events are attributes, but rather the other way round. The 'I' denotes an attribute of successive events. The kernel of this argument is made out to be a mere matter of logic. The logical subject of a series of consecutive contrary attributes must in some sense comprise or include these attributes, and if the identical element in a series of events is an abstraction from them, it cannot comprise them, and hence cannot be their logical subject. This argument seems to rest on the assumption that the relation of subject and attribute is one of comprising, of which the paradigm is an event and its parts. Indeed Harshorne says that events are the final nouns and there is absurdity in the notion that events can be treated as adjectives of something not itself an event.

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## IX.—NEW BOOKS

*The Perception of the Visual World.* By JAMES J. GIBSON. George Allen and Unwin Ltd. London; and Houghton Mifflin Company. New York. Pp. vi + 224. £1 15s.

STUDENTS of the philosophy and phenomenology of perception will find much to interest them in this very original and admirably written book, and even those who do not read it could spend an agreeable and instructive half-hour looking at the excellent photographs and drawings. Philosophers will not perhaps need to concern themselves with some of the more technical details, for example, the functions of crossed double images in retinal disparity. But they can hardly fail to be interested in Mr. Gibson's central thesis. It comes roughly to this: the retinal image plays a far more important part in visual perception, especially in visual space-perception, than is commonly supposed; and there are several sorts of 'stimulus-variables' in the retinal image whose importance has hitherto been overlooked. The Gestalt psychologists made a great point of denying what they called the constancy hypothesis—the assumption that there is a one-one correspondence between the stimulation of receptors and the experience that resulted. Mr. Gibson's aim is 'to reassert the constancy hypothesis on the basis of a broader conception of stimulation' (p. 62 n.). The spatial properties of the environmental world, he says, including its three-dimensional character, 'have correlates in the retinal image, specifically related to their objective counterparts by a lawful transformation' (p. 8); and we react, not just to the direction of light as a plant does, but to a retinal projection of the environment, and hence indirectly to the environment itself (pp. 62-63).

The traditional view was that visual space-perception involves some sort of mental synthesis. It was supposed that retinal stimulation yields only an experience of colour-spots, and that spatial order had somehow to be imposed on those by the perceiving mind; first, they had to be synthesised into two dimensional surfaces with shapes, and then these in turn had to be 'projected'. This was the basis of the traditional distinction between sensation and perception, and also of the traditional controversy between the Nativistic and Empiristic theories of space-perception (Mr. Gibson gives an admirable account of this controversy on pp. 14-25. The only criticism which will occur to philosophical readers is that he completely ignores the influence of Kant, whose treatment of space in the *Critique of Pure Reason* must surely have had a good deal to do with the rise of the Nativistic theory.) The Gestalt psychologists did something to undermine the traditional ways of conceiving the differences between sensation and perception. As a result of their work 'the seemingly vast difference between a sensation and a phenomenal object has been slowly vanishing in recent years' (p. 11). But they still assumed that some process of 'sensory organisation' must be postulated in the central nervous system to account for the occurrence of phenomenal *Gestalten*. They still emphasised the disparity between visual stimuli and visual percepts. It is this disparity which Mr. Gibson denies. 'Order', he says, 'exists in stimulation as well as in experience.' And

he adds that it is 'as much physical as mental' (p. 187). He goes still further. 'The *objectivity* of our experience is not a paradox of philosophy but a fact of stimulation.' He may well claim that 'such a conclusion upsets completely the traditional interpretation of perception' (pp. 186, 187, *my italics*).

To understand how the conclusion is reached, we must consider the distinction which Mr. Gibson draws in Chapter III between the *visual field* and the *visual world*. We wish to know why the visual world looks as it does. Very well: let us look at it. What does it look like? We then find that there are two kinds of seeing. The *visual world* is what we see in ordinary unsophisticated seeing. It is 'the familiar ordinary scene of daily life, in which solid objects look solid, square objects look square, horizontal surfaces look horizontal, and the book across the room looks as big as the book lying in front of you' (p. 26). That is one way of seeing the room in which you sit. But there is another way. 'Look at the room not as a room but, in so far as you can, as if it consisted of areas or patches of coloured surface, divided up by contours'. This sort of looking is not easy. It is of course exactly the sort which the sense-datum philosophers had in mind, though Mr. Gibson does not refer to them. And the instructions which Mr. Gibson gives for achieving it are just those which the sense-datum philosophers would give. 'Fixate your eyes on some prominent point, and then pay attention not to that point, as is natural, but to the whole range of what you can see, keeping your eyes still fixed. . . . It may help you if you close one eye. If you persist, the scene comes to approximate the appearance of a picture' (pp. 26-27). In this new and unfamiliar kind of seeing, you become aware of the *visual field*, as opposed to the *visual world*. 'The visual field, Mr. Gibson says, is 'an introspective and analytic phenomenon'. ('Inspective', I think, would be a better word than 'introspective'.) But still the visual field is *seen*. 'Introspective and analytic' as this experience is, the visual field still depends on light stimulation and a properly functioning eye, as the visual world does.

Unlike the visual world, the visual field has boundaries; it is roughly oval in shape. There is a gradient of clarity in it, from the centre to the periphery. In the visual world there is none; indeed, the visual world has neither centre nor boundaries. The visual field shifts when the eyes move, whereas the visual world is stable. In the visual field, but not in the visual world, 'up and down' is altered by tilting the head, or by lying down on one's side. In the visual field objects are seen to decrease in size as they become more distant, and parallel lines appear convergent. In the visual world objects retain a constant size whatever the distance. Similarly there are two sorts of shape: 'depth-shape' in the visual world, and 'projected shape' in the visual field. Finally, in the visual field one area 'eclipses' another; in the visual world, one object lies in front of another. In short, the visual field, unlike the visual world, has a 'pictorial' quality. It always seems 'a little illusory'. Nevertheless—as the sense datum philosophers too have pointed out—it is not entirely lacking in depth, though its depth can be reduced by such devices as looking between one's legs. I am not sure whether this is entirely consistent with Mr. Gibson's sharp antithesis between 'depth-shape' and 'projected shape', already mentioned. It would seem that there is 'depth-shape', or *some* depth-shape, in the visual field too.

We may now consider Mr. Gibson's treatment of the retinal image, which is the central subject of the book. For a long time, psychologists



and philosophers have been accustomed to insist on the *poverty* of the retinal image. They have thought that it was a very jejune entity, and that many features of visual percepts had no retinal correlates at all. Mr. Gibson, on the contrary, emphasises the richness of the retinal image and the many different sorts of 'stimulus-variables' which can be found in it. We should conceive of it, he says, not as a static picture but as a 'changing event in time'. It is not a picture at all; if it were, we should need another eye with which to see it (for a picture is a special sort of *seen* object). The retinal image is not even a replica: neither a replica of the external object which is its remote cause, nor yet of the visual percept which is its effect. It is only a correlate—a correlate of both, but not resembling either in any literal sense of the word 'resemble'. In this connexion Mr. Gibson has some interesting things to say about the *identity* of the retinal image—the sense in which it remains the same image when it is displaced from one part of the retinal cell-network to another (because of eye movements or head movements) and also the sense in which it remains the same when its shape alters in accordance with the rules of perspective. This rather abstract 'sameness', he suggests, has a good deal to do with the differences already mentioned between the visual field and the visual world. A continually changing retinal image may nevertheless have invariant properties, though some of them can only be detected and described by the use of elaborate mathematical methods. These invariant properties, he thinks, may themselves function as stimuli. They may, for example, be the correlates of the shape-constancy and size-constancy which are important features of the visual world. There are in fact two ways of conceiving of the retinal excitation: (1) as an 'anatomical' pattern, (2) as an 'ordinal' pattern (pp. 55-56). If we think of it in the first way, the same retinal image cannot of course be transposed from one set of retinal cells to another; if we think of it in the second way, it can. Moreover, if the ordinal pattern itself is conceived of in a sufficiently abstract way, we find that it remains identical through changes of shape. Mr. Gibson's hypothesis is that 'the organism can react to an ordinal pattern [of retinal stimulation] as well as to an anatomical pattern'. (P. 56. By 'reaction' here we must understand not just bodily response, but also the generation of a visual percept.)

As we have seen, Mr. Gibson holds that there are far more 'stimulus-variables' in the retinal image than psychologists have supposed. As he puts it, there are various sorts of 'gradients' in retinal images. The most interesting of these is what he calls the gradient of the density of texture. We notice a graded density of texture when we look at a ploughed field. It is a gradient from coarse to fine. From the foreground to the horizon there is a gradual increase of texture-density. Moreover, he points out, there is no difference of principle between a single textured object, such as a grassy meadow or a ploughed field, and an array of what we usually regard as separate objects, such as trees in a wood or a crowd of human beings. If we look at such a set of objects obliquely from above, we can still say that there is a texture density which increases with distance. (This is illustrated by an excellent photograph of a group of closely packed barrels on p. 83.) Corresponding to these texture-density gradients in the visible scene, there are texture-density gradients in the retinal image.

This texture-gradient, Mr. Gibson suggests, is the most important retinal correlate of visual depth, and his discussion of it is perhaps the



most original and exciting part of his book. He thinks that the traditional discussion about 'seeing the third dimension' started off on the wrong foot, as it were. It started from what he calls an 'air theory' of space perception, from the consideration of 'an isolated object or two in empty space' (p. 90). Mr. Gibson prefers a more *terre à terre* treatment of the problem. He points out that we are after all terrestrial animals. Out of doors we are nearly always aware of a 'terrain' which is the constant background of everything we see. When indoors, we are always aware of floors and ceilings. In all normal seeing, we are presented with a more or less continuous surface which is below us, or sometimes above us, and extends 'out and away' from the place where we are standing. Or if, just occasionally, we are not (as when we are immersed in a cloud or a fog or staring upwards at the blue sky) we then find that the perception of depth is absent. Sometimes, he adds, the texture-density gradient takes a special form. It is sometimes a gradient of what he calls *inlines*. There are inlines in a wooden floor, for example (where the floorboards join), or in a pavement, or in a tiled roof or the wall of a house. These inlines are denser in the more distant parts of the object, and less dense in the nearer parts.

Thus we ought not to ask how we manage to see depth *along* the line of vision, as Berkeley did. We ought to ask how we see it *obliquely*: why the floor looks different from the wall, for instance, or why the ploughed field in which we stand appears to recede. Once we put the question in this way, substituting a 'ground theory' for the traditional 'air theory', we notice the gradient of texture-density, which is of course reproduced in the retinal image; and then it strikes us that the retinal texture-density gradient may be a perfectly adequate stimulus for the perception of visual depth. It is true that the retinal image is inverted. But this need not trouble us at all, so long as we remember this is not a picture and is not what we see, but is only a (very complex) stimulus which causes us to see.

In the examples so far given, the texture-density gradient was continuous. But sometimes it is abrupt. There are what Mr. Gibson calls 'steps' in it. Imagine that the grassy field in front of you is level for a certain distance, and then rises up into a hill. Where the change comes from the level surface to the upward slope, the texture-density gradient alters abruptly. From there onwards, the texture-density increases more slowly than it did before, and would no longer increase at all if the hill were vertical. The same thing can be noticed if one looks obliquely at a house. Where the corner of the house comes, there is an abrupt change, a 'step', in the texture-density gradient. (The most prominent feature in the texture here would be the 'inlines' between the bricks.) In this kind of way, the notion of a texture-density gradient can be used to account for the perception of three-dimensional *shape*—depth-shape, as Mr. Gibson calls it. We may notice that this gradient is still present in monocular, as well as binocular, vision.

I do not know whether any previous writer had noticed this texture-density gradient, but when once it has been pointed out it stares one in the face. But there is of course a traditional list of depth-signs or depth-uses, some peculiar to binocular vision (*e.g.* the 'disparity' of the two retinal images) and others (*e.g.* aerial perspective, the distribution of light and shade) which apply to monocular vision as well. Mr. Gibson lists thirteen of them. After a very long and elaborate discussion he concludes that all thirteen can be 'restated as variables of the retinal image', and

that eight of them ought to be regarded as *depth-stimuli*, and not merely as cues or signs whose meaning has to be learned from experience (p. 144).

I do not think that anyone could read this highly intelligent and original book without admiration. But as Mr. Gibson speaks rather severely of philosophers, perhaps a philosopher may be allowed to make one complaint in return. I have already quoted the remark that 'the objectivity of our experience is not a paradox of philosophy but a fact of stimulation' (p. 186). What does Mr. Gibson mean here by 'objectivity'? He seems to mean some sort of *correspondence* between visual percepts on the one hand (especially those which compose the 'visual world') and environmental things and happenings on the other. This correspondence, he thinks, is much closer than psychologists and philosophers have supposed. 'The discrepancies between percepts and objects are not difficult to understand; what we need to understand is why there are so few discrepancies' (p. 43, author's italics). And we can understand it, he thinks, if we can show that many or most of the important characteristics of visual percepts—size-constancy and shape-constancy, for example, as well as depth—are correlated with characteristics of retinal happenings; for these, in turn, as Optics shows, are correlatable with characteristics of environmental objects.

But, if one may ask a very crude question, how does he know about the characteristics of environmental objects at all? It must not be supposed that what he calls 'the visual world' just is the environmental world, though the reader may sometimes forget this. At the beginning of the book Mr. Gibson insists that the visual world, no less than the visual field, is just a product of the human nervous system. What we see at any moment is almost unbelievably complex; yet the whole scene, he says, 'is in every detail a nervous process'. 'The panorama is utterly and entirely a performance of the living organism' (p. 1). In Kantian terminology, Mr. Gibson's visual world is still a phenomenal world, even though it is not just a manifold of sensations. How then does he manage to know so much about Things in themselves—or indeed about the retina, and what goes on in it? This is the kind of question which has puzzled philosophers when they have considered what they called 'the problem of the external world'. Very likely it is a pseudo-question, a muddle or confusion. But it is an awkward and obstinate muddle all the same; and I cannot see that Mr. Gibson's excellent book has done anything to clear it up.

H. H. PRICE.

*The Discourses of Niccolò Machiavelli.* Translated from the Italian with an Introduction, Chronological Tables and Notes by LESLIE J. WALKER, S.J. London: Routledge and Kegan Paul, 1950. Vol. i, pp. xiii + 585; Vol. ii, pp. 390. £5 5s. the set.

THESE two volumes should be read by all who are interested in Machiavelli's political writings, although, because of the high price, only a few will be able to have the pleasure of owning them. The first volume consists of an Introduction of 164 pages, an analytical table of contents, and the translation, to which are appended footnotes consisting of the original Italian word or phrase whenever—and it is quite often—the translator thinks this will be of use to the reader. The second volume consists of 214 pages of notes, of chronological and genealogical tables, annotated lists of Machiavelli's sources, a list

(remarkably small) of his mistakes of history and quotation, and some remarks on his terminology. There are two very full indexes, one of proper names, the other of subjects. Father Walker's procedure in the notes is to give the sources of the historical incidents which Machiavelli refers to, and to discuss whether they justify the practical conclusions he draws from them. He also makes a practice of indicating what Guicciardini said about Machiavelli's arguments in the relevant passages of his *Considerazioni intorno ai Discorsi del Machiavelli*. It is Father Walker's view that the *Prince* and the *Discourses* should be regarded as a single treatise on politics, the former being chiefly concerned with states ruled by one man, the latter chiefly with 'republics', states not so ruled. In his introduction and notes he therefore brings the views of the two books together and in this way aids the study of both of them. This is help for which future students of Machiavelli will be very grateful, for Father Walker has laboured much so that they may need to labour less. Thus, to give but one example, he has not only divided Machiavelli's actual and possible sources into "authors and works mentioned in the *Discourses*", "authors and works not mentioned but certainly used in the *Discourses*", and "authors possibly used or suggested as having been used in the *Discourses*", but has given the editions and dates of each book, indicating whether it was printed or in manuscript. This shows what Machiavelli *could* have read, and should diminish the more unprofitable sort of talk about "influences". Indeed, the scholarly apparatus of this edition is ample, concrete and definite to a marked degree.

Some of those who have rejected the "Old Nick" conception of Machiavelli as the cynical advocate of successful tyranny have gone to the opposite extreme of praising him as the upholder of early Christian virtues in a corrupt age. Father Walker shows that neither of these extremes will do. Machiavelli's attitude towards tyranny may be seen in the *Discourses*, i, 10, where he contrasts Julius Caesar, whom he considers a tyrant, with those few of the Roman emperors "who acted, like good princes, in accordance with the laws". But in the previous chapter he had argued that the extraordinary job of founding or radically reconstituting a state was best done by one person. He held that the Italy of his own day needed this sort of radical treatment, and that is why, in the *Prince*, he discussed the methods it would require. Indeed, what is said about the Roman emperors in the *Discourses*, i, 10, completes, with little overlapping, what had been commenced in Chapter 19 of the *Prince*. On the other hand, however, Father Walker points out that Machiavelli's criticisms of the Papacy were not meant to lead to a renewal of primitive Christianity. For in the *Discourses*, ii, 2, Machiavelli criticises Christian ceremonial as "delicate rather than imposing", and would like to see it replaced by something like the Roman in which, he (misleadingly) alleges, there was "much blood-shedding and much ferocity". In the same chapter he complains that "our religion has glorified humble and contemplative men rather than men of action". When Machiavelli speaks well of religion, it is because of its political utility. In this way he praises the religion of ancient Rome because, although the auspices inspired confidence in the troops, the generals could usually manipulate them as the situation required. One thing that clearly emerges from Machiavelli's discussions of religion is that he takes as his standard of value the good of the state, and that the good of the state includes its expansion. For Machiavelli religion and morality coalesce into patriotism. "I love my native land", he wrote in a letter, "more than my soul". This, and some other passages to the

same effect quoted by Professor Gilbert in his edition of the *Prince*, may serve to confirm what Father Walker says on this subject.

This brings us to Machiavelli's use of the principle that the end justifies the means. Father Walker points out that in the *Prince* there is, for the most part, no question of *justifying* bad means in any moral sense, since in that work Machiavelli is generally concerned to show what are in fact the means to such political ends as consolidating the state after a revolution, irrespective of whether these ends are good. But in the *Discourses* he does put forward a moral principle that could be briefly expressed by the formula "the end justifies the means". The end that, according to Machiavelli, justifies the most evil means is the good of the state, and he holds that any crimes are justified if without them "the safety of one's country" would be jeopardised, and if with them it is in fact secured. Thus he says that Romulus did right to kill his brother because in so doing he intended the good of Rome and in fact achieved it. Father Walker objects (a) that some actions are intrinsically wrong and cannot be justified by any good result whatever, (b) that since good men, to say nothing of bad ones, differ about what is the best political end, action on this principle must be stultifying in its results, and (c) that Machiavelli generally exaggerated the extent to which fraud and cruelty lead to any political good. It should be observed, however, that by making it a condition of justification that the wicked action should *in fact* lead to the intended good result, Machiavelli has confused the moral issue. It looks as though it is the glories of the Roman Republic that justify the crime of Romulus. But Romulus cannot have intended them, and they justify, not him but God or Rome or history. It may be argued that he intended and secured the stability of Rome immediately after the crime. But if that justifies his deed, then most political crimes are justified, for most of them lead to a short period when fear leads to acquiescence. The shorter the period of the justifying good consequences, the less impressive they are, since they may be but transitory. The longer the period, the more they depend on factors the statesman could not have intended, and the less they justify the crime. On the other hand, it has to be admitted that consequences are of more moral relevance to the actions of statesmen than to the action of private persons.

In conclusion I must mention that Father Walker gives a most illuminating account of Machiavelli's attempted construction of a science of politics. He points out that Machiavelli assumes that historical events are repeated in sufficiently similar form to enable laws of historical sequence to be established. He then goes on to formulate a number of axioms about causation which, it seems to him, are implicit in Machiavelli's arguments. And he asserts (vol. i, p. 92): "It was he (Machiavelli) not Francis Bacon, who invented the inductive method", and suggests that Bacon derived his account of inductive reasoning from his study of Machiavelli's writings. I cannot help thinking there is some misunderstanding here. Although Bacon recommended inductive reasoning and attempted an account of its nature, he can hardly be said to have invented it, since it must always have formed part of men's thinking as they went about their practical concerns. Besides analysing certain inductive procedures Bacon urged their application to the systematic study of nature, where hitherto different methods had seemed to prevail. He differed from Aristotle in that the latter did not urge a greater output of syllogisms of the first figure so as to improve the comforts of mankind. If Machiavelli used the inductive method, he did not formulate it, and if Bacon formulated it, he did what

Machiavelli did not do. Whether Machiavelli's writings were a stimulus to Bacon's formulations is quite another matter. Bacon may have seen in the *Prince* and the *Discourses* the detachment, the search for causal laws, and the basing of them on facts that we regard as characteristic of the scientific method. Father Walker gives no more support for this than to say that Bacon had made "a careful study" of Machiavelli. He is right in this, and it is not surprising that Bacon should have greatly appreciated Machiavelli's political maxims. ("He had a delicate, lively, hazel eye; Dr. Harvey told me it was like the eye of a viper"—Aubrey's *Life of Bacon*.) Thus in the *Advancement of Learning* he says that we "are much beholden to Machiavel and others, that write what men do and not what they ought to do. For it is not permissible to join serpentine wisdom with the columbine innocency, except we know exactly all the conditions of the serpent". Later in the same book, in discussing the "sentences politic of Salomon", Bacon praises Machiavelli for supporting his political maxims by reference to real events rather than by fables. Fables, he says, "were vicegerents and supplies where examples failed: now that the times abound with history, the aim is better when the mark is alive. And therefore the form of writing which of all others is fittest for this variable argument of negotiation and occasions is that which Machiavel chose wisely and aptly for government; namely, *discourse upon histories or examples*. For knowledge drawn freshly and in our view out of particulars, knoweth the best way to particulars again. And it hath much greater life for practice when the discourse attendeth upon the example, than when the example attendeth upon the discourse." This is all I have been able to find in Bacon's writings that links Machiavelli with any part of the inductive philosophy. It will be seen that the immediate context relates to political maxims, though the remark about deriving maxims from particulars and then particulars from maxims may have wider significance. It must also be admitted that, just as Machiavelli desired knowledge of political behaviour so as to improve political policies, Bacon desired knowledge of nature so that men might control it to their benefit. I do not know that Machiavelli ever considered what would happen if all or most statesmen became adepts of his new science.

H. B. ACTON.

*The Structure of Complex Words.* By WILLIAM EMPSON. Chatto and Windus, 1951. Pp. 450. 21s.

THERE is a great deal in Mr. Empson's extraordinary book, starting with its title, which is likely to exasperate a logician. Our first impression, on hearing of "complex" words and their "structure", is that we are to expect a discussion of words which are etymologically complex, like "haversine" or "syncategorematic". But of course Mr Empson means nothing of the kind. The complexity and the structure belong, not to words, but to their meaning or use; the rules they obey and the responses they evoke. Mr. Empson's first two chapters are called "Feelings in words" and "Statements in words". A complex word, I understand, is a word which has a lot of feelings "in" it and a lot of statements "in" it.

The structure of Mr. Empson's book is as follows. The first and second chapters explain the conceptual apparatus which, in later chapters, is applied to the interpretation of a number of individual words. Then

there are some more chapters which amplify the conceptual apparatus, and in the last chapter improvements in the manner of writing dictionary articles are suggested. There are appendices on "Theories of value".

"The main thing needed", says Mr. Empson in chapter 1 (page 15), "is to give symbols of their own to some elements often called 'feelings' in a word, which are not Emotions or even necessarily connected with emotions." (I shall recur to these "symbols" shortly.) A word may have several Senses—I follow what is usually Mr. Empson's practice in giving initial capitals to words which form part of his apparatus: and each Sense is capable of receiving various trimmings in different contexts. Two or more Senses may operate at once, in various degrees of co-ordination or subordination. Each Sense may have certain Implications—"part of what is sometimes called the 'connotation' of the word in this use": "thus many uses of *honest* imply 'brave' . . . 'brave' is not a Sense of the word, but is often felt to be present as a Sense". Senses are subject to Appreciative and Depreciative Pregnancy—"a process that makes the meaning of a word warmer and fuller or contrariwise". There are also Moods—"hints of the speaker about his own relations to the person addressed or . . . described". Finally, and a little grudgingly, there is "the Emotion in a word—what is left in the way of 'feelings' when . . . everything connected with a cognitive use . . . has been cut out".

In chapter 2, "statements in words" are introduced. They seem to be divided into Existence Assertions, which "say that what the word names is really there and worth naming"; and Equations. It is Equations which do most of the work in the following chapters. In spite of their name, and in spite of the frequent use of the formula " $A = B$ ", they are non-symmetrical, the transition from " $A = B$ " to " $B = A$ " being only allowed in exceptional cases.

An Equation arises from interplay between the "major" Sense of a word and the "Sense demanded by the immediate context". Let us suppose " $A$ " to be a word which unambiguously conveys the major Sense, and " $B$ " to be a word which unambiguously conveys the Sense demanded by the context. Then the Equation "in" the word is a statement, which the word tends to convey, of the form " $A$  is  $B$ ", or " $B$  is  $A$ ", or occasionally both. I cannot afford space to make Mr. Empson's procedure plausible, but must be content with the bare bones. Here is a quotation from *Lady Windermere's Fan*: "Mr. Hopper, I am very, very angry with you. You have taken Agatha out on the terrace, and she is so delicate." Let us grant, for the sake of illustration, that in its major Sense the word "delicate" stands for feeble health, and that in a given context, here for example, the additional Sense of refinement forces itself on us. The Equation "in" the word may then be a statement to the effect that all refined girls are sickly, or all sickly girls are refined, or both. According as " $A$ " or " $B$ " is subject or predicate we may have Equations of different types. Mr. Empson arrives at five types: but to do so he uses a puzzling piece of jugglery by which *both* Senses may be subject or *both* may be predicate; I cannot follow him here, but it may be be from stupidity.

This is not all. " $A$  is  $B$ " is not necessarily a mere predication. There are the variants " $A$  is part of  $B$ "; " $A$  entails  $B$ " (which I think Mr. Empson does not distinguish very sharply from "all  $A$ 's are  $B$ 's"); " $A$  is like  $B$ "; and " $A$  is typical of  $B$ ". They are all called Equations. I believe this strange proceeding is in substance defensible. The sort of

predicative identification which Mr. Empson is concerned with—as he shows very lucidly in a chapter called “A is B”—is not the sort which has been tidied up to meet the demands of logicians: but the undifferentiated Parmenidean manifold which was there before logicians got to work. It is that, and not the logicians’ product, which operates when, as Mr. Empson sometimes puts it, a word “contains” a doctrine. “The woman is beauty, of course, that’s admitted”, as Mrs. Bloom remarked. At this level we can, without any sense of glaring absurdity, entertain, as though they were the same proposition, “A is identical with B” and “all A’s are B’s but some B’s are not A’s”, and other variants as well.

The equation sign already quoted brings me to the perplexing subject of the “machinery”, which Mr. Empson introduces with a disarming note of apology. It consists of an array of letters, numbers, dots, dashes, quotes, question marks, pluses, minuses, and other signs, which are supposed to designate the Senses, Implications, Moods, Emotions, Assertions, and so on, which a word is alleged to bear in a given context. The method seems to be based on that which lexicographers use in subdividing the senses of a word. For a full dress application of the method we should need, in the first place, a complete Empsonian dictionary entry for a given word, setting out each of its Senses and so forth under the appropriate kind of symbol: and next we should need a quotation, in which the context would impose an interpretation on the word; and this interpretation would be set out in a wholly symbolic formula referring the reader back to various items in the dictionary entry, and showing by its own syntax the manner in which those items interacted—the word’s “structure”, in short. Unhappily I cannot find anywhere a single paradigm of this full dress method of analysis.

In a delightful appendix on Professor L. Bloomfield’s *Language*, and its would-be behaviourism, Mr. Empson remarks that “it is a fine thing that the book should be larger than its theory”, and that “Professor Bloomfield deserves a great deal of admiration for having been able to write against such odds”. Happily Mr. Empson’s book is larger than his machinery. When the reader meets such a sequence of symbols as “ $3c + ?2 = 1a - .1\&1$ ” (p. 89), he will, I fancy, seldom find it easy, after a glance at the definitions, to deduce the meaning with confidence. The machinery creaks, and the pieces in different chapters do not always fit very well. Mr. Empson deserves a great deal of admiration for having written a book of most uncommon interest against such odds. Fortunately no writer is less the prisoner of his own monstrous offspring: the reader need not rack his brains over his unbearable algebra prep, but must be ready to understand Mr. Empson at a wink or a nod.

Mr. Empson’s discussions of individual words are of two kinds. In a number of chapters he examines the use of a word throughout a single work; for example, “wit” in the *Essay on criticism*, or “honest” in *Othello*: and in some places he discusses the general range of a word’s Senses, Implications, and so on, without reference to a limited context. Bits of machinery pop up here and there, but in the particularised discussions they become less and less conspicuous. They do not seem to hamper the working of Mr. Empson’s penetrating intelligence, and indeed I suppose they must help it—perhaps as a private mnemonic.

Even though we may do our best to ignore the symbols, we must try to come to terms with the concepts they symbolise. The guiding idea of Mr. Empson’s work is that the value of great writing lies in its richness



of thought, rather than in hitting the right nerves to produce thrilling sensations and emotions. It is by means of his Senses, Equations, and so on, that Mr. Empson hopes to make explicit what is implicit in our understanding of a single word in a context. In my judgement, his verbal expansions of these implicit bits of meaning, even if in a sense they tell us what we knew all along, enlarge the reader's sensibility in a most striking way.

I think Mr. Empson assumes that quite a lot of words are not "complex" but simple. If that were so, the process of interpretation should be plain sailing. It should be analogous to philosophic analysis, of the classical atomistic kind; bringing us in a finite number of steps to elements not themselves in need of analysis. Now, although there may be "simple" words, I do not think they figure much in Mr. Empson's interpretations. When "knave" and "clown" are used in the interpretation of "fool", or "brave" in the interpretation of "honest", it is not obvious that we have reached a standstill. But the assumption I have attributed to Mr. Empson is perhaps no more indispensable in criticism than it is in philosophy. The good to be won from Mr. Empson's kind of criticism is that by its help we shall become more widely and deeply aware of the thoughts which reverberate "in" the words we use: this good is given to us more abundantly if the words which carry it are themselves reverberative. Perhaps it could not be given to us at all by words which were semantically inert: but I am inclined to think that the supposition that there might be such words is as huge a positivist delusion as the supposition that there are simple natures bearing logically proper names.

A second assumption which I think Mr. Empson makes is that his interpretations might be right or wrong: and certainly one is inclined to think that the Chinese and Japanese pupils whom he quotes sometimes got things wildly wrong. But if so, we seem forced to ask for the criteria of a right interpretation. Here and there, but not often, Mr. Empson gives some countenance to the biographical heresy—the opinion that to understand a work of the imagination is to reconstitute the experiences of its writer; from which it would follow that the ultimate tests of an interpretation were extraneous, lying in such evidence as the writer's diaries; and that the interpretation of the greatest part of literature would be unevidenced guesswork. But in the main Mr. Empson acknowledges, by his practice, that interpretation must arise from the work's impact upon a duly qualified hearer or reader. It is the critic who is right or wrong, rather than what he says. He is wrong if he is stupid or illiterate. But if he is a man of learning, taste, and acumen, and if he tells a different story from another critic, who is neither illiterate nor stupid—well, then we hope that they will put their stories together, or at least refrain from shouting one another down. As Mr. T. S. Eliot once remarked, "there is no method except to be very intelligent".

In a short review, I cannot convey the remarkable richness which is characteristic of all Mr. Empson's critical writings. It is shown, not only in the enlargement of mind which I believe must come to readers of *Lear* or *Othello* who have attended to Mr. Empson's comments on them; but also in his generous domestication, within his own world of discourse, of the ideas of other critics—writers as diverse as, for example, A. C. Bradley, Mr. I. A. Richards, Mr. Wilson Knight, and the late George Orwell. We are obliged to forgive him his machinery, his logical monstrosities, and his almost wilfully erratic use of his own technical terms,



as we forgive Kant his mania for triadic divisions. Philosophers who are serious about finding out how language works cannot afford to ignore Mr. Empson.

I have not noticed many misprints, but the following are bad enough to be worth recording: p. 39, line 3, read "does not show" for "does show"; p. 42, par. 2, line 1, read "causal" for "casual"; p. 205, lines 13 and 14, *probably* read "4/1 + " and "4/1 - " for "4/ + " and "4/ - "; p. 206, line 3, *probably* read "4/1 - " for "4/ "; p. 256, par. 2, line 6, read "good reaction to the whole context" for "good reception . . .".

AUSTIN DUNCAN-JONES.

*The Third Earl of Shaftesbury.* By R. L. BRETT. London: Hutchison's University Library, 1951. Pp. xii + 231. 15s.

THIS book by a professor of English is primarily an account of Shaftesbury's aesthetic theories intended for students of English literature. Hence its sub-title, "a study in eighteenth century literary theory". Professor Brett has, however, related these theories to Shaftesbury's general philosophical views and the thought of the period. He has also included chapters on Shaftesbury's life and influence. The latter was considerable, especially on the Continent where it reached as far as Königsberg and helped, so it is thought, to produce the *Critique of Judgment*. The result of Professor Brett's work is the first relatively comprehensive account of the life, writings and influence of the founder of the school of Taste in ethics and aesthetics. As such it deserves a welcome also from philosophers. This may be qualified for some by the author's excessive fondness for Kantian epistemology, including its incantatory transmission by that arch-mystifier, S. T. Coleridge. This leads him sometimes too easily to seem to detect in Shaftesbury's fairly simple doctrines, advance whispers of transcendentalism. It also leads to a rather simple acceptance of the array, Sense, Feeling, Understanding, Imagination, Reason which are conjoined and opposed as friendly and warring elements to, one regrets, no very illuminating purpose. It seems a pity, now that most philosophers and psychologists have emancipated themselves from superstitions about mental faculties and functions, that they should have left these fossils to mislead the intelligent public. It is even worse that these words have been used to stand for avenues to knowledge of different kinds of facts and objects, so that Professor Brett not only expounds but defends Shaftesbury's antagonism to the empiricist philosophy of Hobbes and Locke because it could not account for 'non-sensory' knowledge, involved even in perception, so not for knowledge by Reason or Imagination and thus not for poetry and art. For, as Shaftesbury is credited with realising, "the symbolism of the poetic imagination can be as true as conceptual statement" (p. 28). The argument seems to be that since poets do not (admittedly) write only about what they now perceive and feel or can remember having perceived and felt, they must write about some other type of fact for which they invent special symbolisms by a peculiarly 'creative' use of Imagination, sometimes combined with Reason (as opposed to Understanding). Only, Professor Brett seems to think, some such account can justify an objective standard of beauty. But is it not obvious that this is an answer designed to fit and therefore unable to solve the aesthetic, or any other, problem?

As with the similar answer in ethics, no more can be said of these facts about which poetry is true nor, of course, does Professor Brett consider the application of this theory to other arts though, presumably, the poet is not unique among artists. There are genuine problems of aesthetic value and artistic communication but they can be tackled only by a careful examination of the use of such words as 'good', 'bad', 'imagine', 'create', etc. in connexion with particular works of art, when it may be found that such words as 'Imagination', 'Creation' and the rest cover whole families of differing and resembling facts and do not stand for any simple faculties or modes of revelation. A good beginning might be to bury Coleridge finally as a pseudo-philosopher. As such he has little but muddle to contribute to aesthetics, whatever his merits as a literary critic.

Nevertheless, Professor Brett makes some interesting points about the relation between Shaftesbury and his former tutor, Locke. Shaftesbury was mainly interested in morals and art. Locke's ethics are an unsatisfactory mixture of crude self-interest and barren rationalism. Culturally, Locke seems to have been unusually philistine. His educational precepts include none for the cultivation of aesthetic sensibility. One suspects his educational practice did not better his theory. Nor does the Essay mention such ideas as those of art and beauty. It was chiefly on these issues that Shaftesbury differed from Locke. Professor Brett's book rightly emphasises that the doctrine of Taste did at least show that men talk otherwise about art and morals than about science and mathematics. For this we owe a debt to Shaftesbury.

Among the most interesting chapters of the book are those on purely literary and aesthetic topics such as Shaftesbury's doctrines of the Sublime (chapter VII) and of Ridicule (chapter VIII). Shaftesbury held that ridicule is a test of truth. What is false and unreasonable will not long survive being laughed at. Laughter is a potent weapon against vice and folly. Shaftesbury was, of course, furiously attacked by the earnest for this wise and tolerant doctrine, but it was applied with vigour by the Augustan writers of comedy and satire.

On the whole an interesting book. For its philosophical demerits perhaps philosophers are themselves to blame. They can only issue a warning against all philosophical terminologies!

MARGARET MACDONALD.

*The Individual and his Religion.* By GORDON ALLPORT. London, Constable, 1951. Pp. xiii + 163. 12s. 6d.

THIS book is a revision of the Lowell lectures delivered in Boston in 1947 and presents not a systematic treatise but a series of comments "directed solely to the portrayal of the place of subjective religion in the structure of personality. My approach is psychological, some would call it naturalistic. I make no assumptions and no denials regarding the claims of revealed religion." These comments are often interesting, e.g. the discussion of faith in terms of forward-looking goal-directed activity rather than of its psychological roots; sometimes clarificatory, as in the insistence that the origins of a belief are not (logically) relevant to consideration of the validity of the belief itself; and occasionally facile, as his brief reference to science and religion and the axiom of determinism

which "has never been proved in any absolute sense" and is rejected by the scientist outside his laboratory when he "praises and blames, admonishes and exhorts . . . none of these activities consistent with the axiom".

Professor Allport first discusses the multifarious origins of religious belief, rejects the possibility of characterising a single basic form of the religious attitude, prefers to talk of the "habitual and intentional focusing of experience" and always stresses the individuality of each man's religion. There follows a chapter on the Religion of Youth, with a report on a survey of the religious beliefs of a sample of Harvard students carried out in 1946-47. From this it is concluded that religion is still an important psychological phenomenon, but it is here evident that "religion" is being understood as including "an ethical but not a theological Christianity" and not inconsistent with, *e.g.* Dewey's attitude that "religion should have nothing to do with supernatural notions".

The most important part of the book is that on "the Religion of Maturity". Psychological criteria in terms of emotional adjustment, capacity for self-objectivisation, integration, etc. are applied to distinguish the mature religious sentiment, which becomes, broadly speaking, individualistic, tolerant, critical, prepared to face awkward questions. This is contrasted with the fanatical, which is "fed by immature urgencies arising out of unconscious forces" and the infantile, which is largely concerned with "magical thinking, self-justification and creature comfort". Such a mature religious sentiment which, whatever its origins, becomes "functionally autonomous" (a concept dealt with at greater length in the author's "Personality"), is defended against charges of wish-fulfilment, father-fantasy, etc. and, it is claimed, may play a legitimate, indeed vital, part in the working of a fully-developed personality.

It seems that a psychological norm (acceptable in many ways to western liberal tradition) is being used to establish a religious norm (acceptable in particular, perhaps, to certain kinds of low church Protestantism). Clearly such norms have their uses and Professor Allport is cautious in his claims for them. Each man is himself, and in an aside considering the suggestion that many religious leaders are clearly borderline psychotics, he remarks "unless one deviates in mental type from the prevailing norm one can scarcely be creative or describe new horizons". But why should such norms be accepted by the defenders of varying creeds? Many a cottager or saint seem to be in danger of being convicted of mental under-development or emotional maladjustment (the subject of mysticism is touched on and proves difficult, although it is admitted that mystical experience is not incompatible with the mature religious sentiment), which may be psychologically interesting but is not necessarily connected with saintliness or the lack of it. (The point seems parallel to the difficulties of intelligence measurement: a Civil Service Intelligence test may not be useful for selecting candidates for an Art School.) Allport rejects what he calls the genetic fallacy, but is still infected with it since it seems that, *e.g.* evidence of unresolved conflicts in a man would detract from the value of his religion. Although it is said that value judgments are not to be got from the laboratory, the term "maturity" applied to religion carries an evaluative element which may be rejected with good reasons by the priest, who has methods of evaluating human personality and behaviour, which may never be the business of the psychologist in his professional capacity.

Thus, although there is much that is valuable in Allport's suggestions of the way in which priest and psychologist can co-operate, and illuminating in his comparisons of Christian ethical teaching with psychological discoveries, there remains the possibility of a fundamental divergence between the two.

W. E. H. WHYTE.

*The Nature of Law.* By T. E. DAVITT, S.J., Ph.D. B. Herder Book Co., St. Louis and London, 1951. 30s net.

THIS is a disappointing book. The author—an American Jesuit on the staff of the St. Louis School of Law—follows the general Thomist line. It might have been expected, therefore, that he would seek to vindicate the notion of justice as an objective standard and of the natural law as the rational foundation of all legal systems, and endeavour to show how such an approach is compatible with the immense differences between systems growing up in varying social and economic environments. Such speculations would find some common ground in two recent collections of lectures, the late Professor Harold Potter's *Quest of Justice* and Professor F. H. Lawson's *Rational Strength of English Law*.

Instead, the book is taken up with a sterile and abstract controversy as to whether "intellect" or "will"—pictured almost as separate entities—plays the greater part in the making of law. The treatment is historical, but limited to the medieval period: on one side are ranged Duns Scotus, Ockham and Suarez, on the other Thomas Aquinas, his predecessor Albert the Great, and his interpreter Cajetan. The only value of the book, therefore, is as a study of a scholastic disputation devoid of present-day interest. Fr. Davitt completely disregards the facts of legal systems (modern or medieval). He insists on treating law as nothing but an ordinance or command—a view which in theory can be made to fit the continental codes with their reliance on the maxim *Quod principi placuit legis habet vigorem*, but which, in Anglo-Saxon countries, ignores the vast range of judge-made law founded on reasoned principles of justice. Maritain, one of the few profound thinkers of the Thomist revival, has not missed this point, and suggests in his *Premiers Principes de la Morale* that law is primarily a norm or standard of conduct and only secondarily a command.

If Fr. Davitt's talents lie in historical research rather than in original speculation, he could usefully turn his attention more thoroughly to the great and neglected figure of Suarez. Suarez is interesting as a philosopher, because he was independent and critical and always insisted on careful inquiry into the facts—qualities not usually associated with the later scholastics. His permanent importance, however, does not lie in the abstract theories sketched by Fr. Davitt, but in his role as one of the founders—before Grotius—of international law, where his views on the unity of nations, the limitation of sovereignty, judicial arbitration between states and the responsibilities of a colonial power have a strikingly modern ring. A study of the work of Suarez on international law and its influence would be of great interest.

One further small point: it is inexcusable for a legal scholar (at p. 106) to refer to "land rent" as a form of medieval taxation when what he is referring to is quite obviously feudal dues.

JOHN MUNKMAN.

*St. Thomas Aquinas—Philosophical Texts.* Selected and translated with notes and an introduction by THOMAS GILBY. Geoffrey Cumberlege, Oxford University Press. 12s. 6d.

SHOULD expositors of the philosophy, as opposed to the theology, of Aquinas adopt the *via inventionis*, as recommended by St. Thomas himself, or the *via iudicii* of the *Summa Theologica*? The second alternative is neat but theoretically improper, the first proper but not neat, involving as it does an "ascent" through much obsolete natural philosophy. Fr. Gilby has tackled the dilemma by presenting these philosophical texts in the order of the *Summa*, yet prefacing this general scheme with a set of extracts which follow the *via inventionis*. Such a compromise facilitates pruning of waste matter while, for instance, some of those pithy remarks on the status of scientific hypotheses are preserved: (why has the prophetic specimen from *De Caelo* 1. II, c. xii, lec. xviii not also been included?). The widest possible range of works, from *Summae* to *Opuscula*, is represented in the texts, which are in fact numbered and cross-referenced passages varying in length from a couple of lines to a couple of pages. Very occasionally the cogency of the cross-references is a little obscure, or it may be that slips have occurred: thus, should not extract number 448 be correlated with 197 instead of with 196, and 447 with 196 instead of with 197? In some cases too, the references are not reciprocally quoted, as when 448 refers to 196, but the latter to 432 only. Such rare lapses, however, scarcely detract from the selections' indisputable value as a sample-library of St. Thomas's writings on any given topic.

At times St. Thomas is made to speak in the fashion which Fr. Gilby made his own in the breathless pages of *Barbara Celarent*, as when we have "[he is] acting as a wet blanket" for "*aliorum delectationes impedit*" (1004), and "by borrowing and not by owning" for "*per participationem et non per essentiam*" (175): the last example might cause trouble if taken too seriously. However, "communal sense" and "factive intellect" are instances of improvements on the usual versions of old acquaintances, especially the second, penetrating as it does the latin curtain. A rather staccato impression (due to the spacing of the often isolated extracts), is compensated to some extent by the context which headings, grouping, and notes supply, as well as by the artifice of choosing as far as possible texts so worded that they become continuous in sense with their predecessors or successors, even though these are from quite diverse sources (e.g. 359 and 360, 481 and 492). In certain more familiar passages one becomes conscious that we have, in the translator's own words, "a compromise between a paraphrase and an exact and literal rendering": pronouns replace proper names, sentences are moved or omitted, and paragraphs re-defined or re-arranged. The texts are supported by numerous footnotes and also by what is at times a rather lightly-skating introduction. Did Siger de Brabant *really* become an "admirer" of Aquinas? Stocky and well indexed, this volume will doubtless be diversely appreciated by diverse sorts of people, and likewise diversely used or misused.

D. P. HENRY.

*The Logic of Personality.* By BERNARD MAYO. London: Cape, 1952. Pp. 188. 10s. 6d.

MR. MAYO is probably right in claiming that philosophers have had little to say on his subject. His own approach is modern in that it tends to be anti-intellectualist without leaning to any sort of mysticism. For example, in discussing how to decide whether certain people have the concept of 'personality', he emphasises (like those anthropologists who make ritual prior to myth) that action commonly determines belief and not *vice versa*. Similarly when he deals with his crucial topic, the nature of 'acquaintance', Mayo considers the behaviour of an ideal 'primitive organism' and an ideal 'primitive man'; and if this sounds a hazardous combination of genetic and analytic points of view, I can only say here that, to my mind, it came off successfully. One of the points made from the biological angle is that awareness is of changes, not states; and this is used as an argument against sense datum theory. The book contains plenty of interesting things like that. And whether or not they are cogent they are always clearly presented. They include a fairly long consideration of some ethical questions.

Mayo's actual thesis about 'personality' raises a number of doubts. The term is commonly used for the property of being a person or individual, and secondly as a synonym of 'character', which is a set of properties: but Mayo is in effect demanding a third meaning that cuts across this distinction. For the second meaning is dismissed on the ground that no amount of knowledge by description is knowledge of personality. This point is taken for granted: but surely it could be denied? If Mayo thinks he is defending people's ordinary way of talking and thinking, some of them will hardly thank him for his defence—we should be *chez Swann* with a vengeance, since it implies that a man is as many persons as he has acquaintances. For something possesses individuality (he argues) inasmuch as someone is acquainted with it; this relation is often expressed by calling something 'my' table, 'his' garden and so on; and it always holds between an artist and his subject. Mayo objects to the usual account of knowledge by acquaintance on the ground that acquaintance implies familiarity or recognition: but he seems to give no reasons for his denial (p. 31) that these "involve any generality, any extension beyond the particular". Or take the 'individuality' of Van Gogh's *Yellow Chair*. No set of *properties* will account for it, or Mayo's thesis. But might not that be true only if one thinks of properties ordinarily associated with chairs? Suppose one was thinking by means of a chain of symbols representing such properties, but at the same time by means of *other* symbols too (feelings and musings) which might be of anything from the yellow of summer corn to a tactile and sexual sensation of stroking. Might not the crossing of these chains produce so powerfully condensed a symbol that we might misname it absolutely personal and ineffable? (On p. 125 Mayo himself notices, without removing, the difficulty that some of his 'personal relations', all of which should be unanalysable, seem to be complex.)

There are no footnotes, and Messrs. Cape have given another lesson in book production.

A. C. LLOYD.

*Religious Faith, Language and Knowledge. (A Philosophical Preface to Theology.)* By BEN F. KIMPEL. Philosophical Library, New York. Pp. x + 162. \$2.75.

PROFESSOR KIMPEL's book is a criticism of what he calls (perhaps not very aptly) "empiricism", the view that we know only our experiences and not the "reality" that they are experiences of. Contemporary philosophers of language are "empiricists". "Contemporary empiricists in language analysis" assume that "statements are all that we can know" (p. 39): they deny a reality beyond language, which is to say that they deny a reality beyond experience. Contemporary trends in philosophy are, therefore, anti-religious; for religion and theology are concerned with a reality beyond experience. Professor Kimpel as a religious believer wants to put in the place of "empiricism" what he calls "realism", which he explains as the view that we have knowledge of reality beyond experience. But this knowledge, he holds, is really a kind of faith, and he draws the not unfamiliar analogy between knowledge of God and knowledge of the external world. Belief in the existence of God and belief in the existence of a "physical reality" are both matters of "faith": they are not immediately given in experience. The second of these beliefs he thinks essential for a scientist. So science is based on faith. Religion also is based on faith, *but not more so*. It follows for Professor Kimpel that if science is considered respectable by the contemporary philosopher religion should be admitted to be so, too.

Professor Kimpel's writing is not over precise (for instance, can we be said to "know our experiences"? and what, anyway, is experience? and what is meant by "statements are all that we can know"?), and there are some steps in the argument that one would wish to see expanded; but he succeeds in putting across his main points with force and enthusiasm. He seems to me, however, to mistake what is being done by contemporary 'linguistic' philosophers, possibly in part because he makes no distinction between different schools and generalizes too much from the views of some Semanticists; though even the Semanticists are not the more-than-positivists he thinks them. To say, as he does, that any philosopher who is concerned with the clarification of utterances can speak only about words and never about reality is misleading; for to "clarify the meaning of statements" (p. 8) is to "interpret the nature of a reality other than language" (*ibid.*)—to interpret it 'from a new angle', perhaps, or 'in a new way', but certainly to interpret it. Otherwise, why do it? Linguistic philosophy is not just a game. Professor Kimpel seems almost to think that it is; and, at that, a game devised by the devil.

THOMAS MCPHERSON.

*The Recovery of Belief.* By C. E. M. JOAD. Faber and Faber, 1952. 15s.

THOUGH described as 'A Restatement of Christian Philosophy' this book mentions neither Augustine nor Aquinas. It deploys the considerations which have led Dr. Joad, reluctantly, from a fairly aggressive disbelief to a rather half-hearted faith. Yet readers of his earlier books will find no new arguments here: only the familiar material—with its familiar merits and demerits—reshuffled into a different pattern—under the impact of his realisation of how very evil men can be.



He is careless about facts and authorities. One would like to know who estimated that "throughout most of recorded history the human population . . . has stood at approximately five hundred millions" (p. 26); and that "In 1950 there were over three million illiterates in Great Britain" (p. 75). He asserts, "Mystics . . . unlike those in whom a special psychological condition has been induced by opium, hashish or other drugs, do claim a kind of knowledge" (p. 104). But precisely such claims are in fact made under mescaline, nitrous oxide, etc.: compare, e.g. William James's *Varieties of Religious Experience*, chapter 15, on "the anaesthetic revelation".

He is curiously unaffected by the philosophical literature of the last twenty years. Since he simply assumes that "the law of induction" forever demands an unattainable "validation" (pp. 93 ff.) it is not surprising that he neglects off-the-beaten-track publications such as *Socratic Digest* and *University*; though Miss G. E. M. Anscombe's 'A Reply to Mr. C. S. Lewis's Argument that "Naturalism" is self-refuting' in number four of the former undermines a large part of the book, especially chapters 4 and 5. Again, accepting arguments that the naturalistic fallacy is a fallacy, he can find "no way of avoiding the conclusion that ethical qualities exist and that ethical judgements refer to them" (p. 148): and hence that "we must postulate a sphere of 'what is' into which science does not and cannot enter" (p. 149); this "non-natural order" is taken to be "supernatural" (p. 78). Finally, in spite of all arguments from Hobbes, Locke, and Hume onwards, he takes it for granted that free will and determinism are antithetical antagonists (pp. 136 ff.).

All in all this is a rather pathetic book.

ANTONY FLEW.

*A Philosophical Essay on Probabilities.* By PIERRE SIMON, Marquis de LAPLACE. Translated by F. W. Truscott and F. L. Emory. New York: Dover Publications, Inc., 1951. Pp. viii + 196. \$1.25.

THOSE of us who welcomed Basil Blackwell's enterprise in producing a cheap reprint of Boole's *Mathematical Analysis of Logic* will wish to congratulate also the publishers of this useful paper-bound volume. With Laplace as with Boole, it is a joy to go back to the beginnings of a subject which has since become over-technicised, and to see clearly once more the outlines of the philosophical problems it raises. Many of our troubles about probability can be traced back to this classic essay of Laplace's, and perhaps if we approached them with his urbanity we should come to find them less distressing. It is illuminating, for instance, to find him speaking of the ratio 'favourable/all cases', first, as the *measure* of the probability of an event (p. 7), but later, for the purposes of his axiomatic theory, as the *definition itself* of probability (p. 11): he at any rate did not think of this ratio as an exhaustive synonym for the words 'probable' and 'probability'. Not the least useful thing in the essay is the historical survey of the subject with which it ends (pp. 185 ff.): other things of special interest are the excellent discussion of the application of the calculus of probabilities to natural philosophy (chapter IX), and the curious attempt to assess 'the probability of the decisions of a parliamentary assembly' (pp. 126 ff.), in which it is assumed that



the business of assemblies is to vote on such questions as, what is in fact going to happen.

The present translation is mostly intelligible, but it is not good. Too often French constructions are retained, with a mere substitution of English words for French ones. Sometimes the French words are simply re-spelt, with loss of sense ("the incident will be discredited by enlightened auditors", "the vacillations of hazard", etc.). In one instance this carelessness is crucial: *esperance*—an important word for Laplace—here means something nearer to expectation than to hope. Elsewhere it results only in a feeling of clumsiness, as we continually have to read 'mathematician' for the printed 'geometrician' and so on. Names are another trap: Ticho-Brahé, Moivre and Oldembourg are transparent disguises, but I had to turn up my works of reference to check that the classic papers of Bayes and de Moivre had in fact appeared in our own *Phil. Trans.*, and not in a Parisian publication.

STEPHEN TOULMIN.

*Other Minds.* By JOHN WISDOM. Oxford: Blackwell, 1952. Pp. 259. 21s.

*Philosophy and Psycho-analysis.* By JOHN WISDOM. Oxford: Basil Blackwell, 1953. Pp. vi + 282. 22s. 6d.

*Other Minds* contains the series of articles on that topic which was originally published in *MIND*, together with three papers on the same topic which originally appeared in the *Proceedings of the Aristotelian Society*. In these articles Wisdom examines the problem of what sort of knowledge we can have of the minds of others and how we can get it; his method is to set out exhaustively the reasoning which leads to each of the well-known philosophical positions on this subject. One might add that he sets out also the difficulties in each view, though this is not a separate thing, for the difficulties in each view are the reasons for some other view. Wisdom does not finally embrace one of his theories; the idea is that all that can and need be done for us is to make us see clearly why we wish to hold these paradoxical theories, and thus comprehend the vision, and complementary blindness, of each. We end with understanding, not with a theory. In the grand design Wisdom fails with at least one reader, as many great philosophers have failed before him. But in the process of failing in the overall design he succeeds in many subsidiary discussions. Thus the examination of scepticism about the possibility of empirical knowledge seems to be incomparably the best we have; we can learn enormously about analogy; and the footnote to the first page contains a greater concentration of philosophical wisdom than one usually finds dispersed over many weary pages. But it must be assumed that all readers of this journal (except those who won't see) have long ago found this out for themselves.

*Philosophy and Psycho-analysis* contains a wider variety of essays by Wisdom, most of them reprinted from journals, though not all readily obtainable except from the biggest libraries. We start with two articles from the period of straightforward analysis of facts, the heyday of the complex but clear-cut logical construction. (One can see that considerations of space prevented the reprinting of the *Logical Constructions* articles from *MIND*, but it is a pity for they were full of good things.) Next comes *Philosophical Perplexity*, a true landmark in the history of modern British philosophy, for it was the first exposition of the view of

philosophy which was to supplant, for good or ill, the position which the first two articles embodied as being, with local variations, typical in British (English ?) philosophy. Though Wisdom always acknowledges his debt to Wittgenstein, it is Wisdom who made the view known to more than a small circle in Cambridge; and it would be a great mistake to think that Wisdom did no more than disseminate. The remainder of the articles are examples of the method in action, as are the *Other Minds* articles. A number of them take as a main or prominent theme the comparison *and contrast*—facile scoffers at 'therapeutics' should note the last two words—of the aims and methods of the analyst in philosophy and psychology, from which fact the title of the book is derived; but theology, probability, and many other topics have their place in the collection of essays.

It is good that these essays by Professor Wisdom should be made easily available to those who have not long rows of journals on their shelves; right or wrong, they are examples of first-rate philosophising; and, though colloquial to a point where grammar is perilously strained, they have often a strange literary beauty. But it is sad that when photostatic reproduction is carried to the extent of incorporating uncorrected the page references of the original articles it should be found necessary to make such heavy demands on the would-be purchaser's pocket. If it is too much to ask for uniformity of type for 22s. 6d., surely we may at least expect some rectification of these details.

J. O. URMSON.

*Medieval Logic. An Outline of its Development from 1250-c. 1400.* By PHILOTHEUS BOEHNER, O.F.M. Manchester University Press, 1952. Pp. 127. 12s. 6d.

THIS book does not pretend to do more than barely justify the promise of its title. As the author says, the logic of the period is still largely unexplored. That is due to a lingering tradition of anti-scholasticism among non-scholastics, to a rather low general standard of logical culture among neo-scholastics, and to a lack of training and interest in logic among medievalists. However, this situation is slowly being improved, and Fr. Boehner's book should help the movement on its way.

Interest is centred on two subjects, the theories of Consequences and Supposition, these being regarded as the logically most important medieval amplifications of the Aristotelian tradition. The form in which this tradition was received in the period under review is well seen in the commentaries of Albertus Magnus, briefly summarised here for this purpose. The example is a fair one, for Albert's mind was certainly encyclopaedic rather than systematically speculative, and his work was chiefly of value as a medium of transmission, not always in very intelligible form. It was in elaborating this corpus of received teaching that the thirteenth- and fourteenth-century scholastics developed their characteristic treatises on *syllogismata*, suppositions, insolubles, consequences and the rest. In so doing they believed themselves, as is rightly emphasised, to be effecting a genuine development that was not in any way a revolution. How far they were consciously in touch with Stoic achievements is still an open question; Boehner suggests a link in the technical use of the word *syncategorema*.

The origin of the scholastic theory of consequences, that theory which we now call propositional logic, or theory of inference, is hesitatingly

traced to reflexion on the *Topics* of Aristotle, with some influence from Boethius's *De Syllogismis Hypotheticis*. Such discussion as there is of this point is very obscurely formulated. That Boethius cannot be considered a major source is probably correct, for had the totality of his data been accepted the scholastic theory of consequences must have been much richer than seems to have been the case. Reasons adduced in the same sense by I. M. Bocheński in *Angelicum* XV (1938) also seem powerful. But it is more doubtful whether Aristotle's *Topics* are the genuine source of this treatise. By Boehner's admission it grew out of additions to the topical rules rather than as a direct development. Bocheński (*loc. cit.*) emphasises rather the influence of the *Analytics*, and the reviewer would suggest the *Elenchics* with its stress on the importance of the *fallacia consequentis* as a likely major influence. It is in his treatment of that subject that Peter of Spain shows the emergence of consequence as a clearly grasped relation.

The largest body of consequences so far gathered from any single author is Salamucha's list from Ockham. A portion of it is given here and the tempting project is undertaken of trying to reconstruct some of the processes of thought that led from one to another. Some corrections are needed in these pages. The rules quoted at the top of page 60 refer to C 1b and C 1c, not to C 1a and C 1b. Two attempted reconstructions are given of Ockham's deduction of the rule corresponding to

$$C\ 5a\ (p \supset q) \supset [(r \supset p) \supset (r \supset q)]$$

from that corresponding to

$$C\ 4a\ (p \supset q) \supset [(q \supset r) \supset (p \supset r)]$$

For the first, the thesis

$$C\ 1c\ (\overline{p \supset q}) \supset (q \supset p)$$

is required and stated to be an equivalence, which is not the case. The second is somewhat too sketchily set out and its result is inaccurately described. The process is to assume 5a false whence by correct steps one can deduce  $(p \supset q) \supset [(r \supset q) \supset (r \supset p)]$  which Ockham rightly rejects but which is not "a contradiction of 4a". Greater formality would have prevented these errors.

The treatises on supposition are dealt with predominantly as embryonic theories of quantification. Without denying anything that the author says on this subject, it should be added that they are very much more than that, and the distinctive character of medieval logic will not be grasped until one has understood the formality under which so many, and to the modern mind such diverse, topics were treated of with this single title. A properly historical study should not be content to see how much of a later stage of development was contained in an earlier, but should also endeavour to see that earlier as its contemporaries saw it. The subjects dealt with under the heading of "supposition" have now been parcelled out among various branches of logic, but something essential has been missed if it is not appreciated how they were originally envisaged as one subject. It seems likely that a good deal of work on medieval logic lies immediately ahead, and investigators need to be careful not to see their material only through a modern stencil.

One is left with the impression that the author has started his period rather too late and ended it a little too early. He is indeed acquainted with the work of Grabmann on the period Abelard to Peter of Spain, and knows that much of which he writes has its roots there, but the general

tendency is to stress the fourteenth century, whereas the real historical interest lies in pushing back as much of this material as possible to the earlier years; e.g. materials for C 1d  $p \supset q \equiv p\bar{q}$  are to hand in Kilwardby c. 1240. Again, some remarks are made about the breakdown of the movement "in the sixteenth century". This is seen as part of a general decline in scientific culture and is attributed to "well-known exterior reasons". However, some details of the breakdown would not be without interest. Joannes Argyropulus, writing his *Dialectica ad Petrum de Medicis* about 1457, speaks of the treatises on supposition as *tam longam atque obscuram materiam* which *ad usum oratorium affert pene nihil*. It would be worth while to establish the state of affairs in the preceding sixty years.

Discussions in this book are not always very clearly phrased. At times Latin terminology is too closely followed, as when *infern* is translated "infern" rather than "implies"; while on the other hand the Latin text in note 103 is carelessly turned, for "provided that" makes nonsense of the English and nothing corresponding appears in the original. Notwithstanding criticisms of detail, the book strikes an original note in the history of logic, and enriches the possibilities of future research.

IVO THOMAS.

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## THE CONGRESS OF METHODOLOGICAL STUDIES IN TURIN

It may be of interest to the readers of this journal to hear that a group of Italian scholars has organised and very successfully held a Congress of Methodological Studies in Turin on 17th to 20th December, 1952.

If an analytical philosophy such as the one started seventy years ago by G. Vailati and promptly wiped out by the triumph of neo-idealism is going to spread in Italy during the coming years, reference will be probably made to this Congress, as to its first public event. 174 people, ten of whom foreigners, asked to be enlisted, and there were 66 contributions delivered in four parallel Sections (General Methodology, Methodology of Mathematical and Natural Sciences, of Social and Legal Sciences, of Work Organisation and Human Relations). Quite a few questions to be discussed within one of the Sections or in additional general meetings were sent beforehand to all enlisted persons.

Against the background of Italian traditional philosophy, the word "methodology" has come to mean something like what is meant by "linguistic" or "analytic philosophy" in English-speaking countries. There has been a remarkable and perhaps unexpected interest in questions of language as important for philosophical problems. It is worth while adding that little reference was made to symbolic logic in spite of the fact that the general tendency was more for linguistic reconstruction than for colloquial analysis or therapeutic techniques. It will be interesting to see what sort of philosophy can be produced by scholars equally removed from both main sides of contemporary linguistic philosophy, and still concerned with language.



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